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MODERN ASTROPHYSICS

PLATE I.

Portion of " North America " Nebula.

Nebula South of ζ Orionis.

Frontispiece.

MODERN ASTROPHYSICS

by

HERBERT DINGLE

Fellow of the Royal Astronomical Society ; Fellow of the Physical Society of London ; Assistant Professor of Astrophysics at the Imperial College of Science and Technology.

This World must necessarily be the resemblance of something—although to describe its origin according to Nature is the greatest of all undertakings.

PLATO, *Timæus.*

New York
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1927

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PREFACE TO THE FIRST EDITION

A SCIENCE in its infancy is the least satisfactory, and, at the same time, the most profitable theme for a general description. It is the least satisfactory because its conclusions—if we can call them conclusions—are, at the best, little more than tentative summaries of observed facts, liable at any moment to be superseded by wider generalisations: the inconsequential playfulness of childhood has not given place to the graver consistency of mature age. It is the most profitable theme because it has not yet lost the quickening inspiration that alone can produce great things. It is in touch with the poetry and romance that go side by side with all true science. In its eyes still shines "the light that never was on sea or land."

This book is intended primarily for the general public. The subject of Astrophysics is presented as a panorama, seen from a detached point of view with vision characterised by breadth rather than by acuteness. An attempt has been made also to convey indirectly to the reader something of the emotions of the practical worker—something of the strange mixture of enthusiasm and wonder, assurance and distrust, eagerness and fear; of amazement at the splendour of his achievements, and humiliation at their meanness; of delight in his knowledge, and disgust at his ignorance; of confidence for the future, and shuddering at its difficulties; in short, of the "divine despair," that agitates the breast of the modern astrophysicist when he surveys the untrodden land into which he has pierced. Astrophysics is the most imaginative of the sciences. Is it too much to hope that one day it might be the means of revealing the essentially imaginative spirit of all science?

While the popular appeal has been given first place, difficulties have not been evaded. He who runs will read nothing of value in Astrophysics. It has been assumed here that the reader is prepared to think. No specialised knowledge is required; no technical or unfamiliar language is used without explanation; mathematical formulæ, when they give more precision to the statements of the text, have been

placed in footnotes, and may be ignored by the non-mathematical reader: but, as far as possible, reasons have been given for the ideas presented, and, in order that those reasons may be followed, careful reflection is necessary. Limits of space demand that the subject-matter shall be treated in the briefest possible way: more than once a few short sentences have had to carry the burden of a weighty argument—not, however, it is hoped, with any loss of clarity. Nothing has been omitted because it is difficult: nothing has been included because it is sensational.

It is believed that the book is sufficiently comprehensive to be of use, as a review of the present position, to the practical astrophysicist. How inadequately it meets the demand for such a summary is to no one more obvious than to the author. His chief reason, indeed, for hoping that it might be of service in this respect is that nothing of the kind now exists in the English language. The book makes no claim to the all-embracing view, so urgently needed now, that would make it an interpreter of the present and a guide to the future. It is simply an ordered collection of knowledge—a fabric woven of the discoveries and ideas that are believed now to be of value, through which the ghost of a diffident, but well-meaning, criticism may be observed occasionally to peep. It can have, at the best, a transient usefulness; but, until the fuller work comes to be written, it is hoped that it may have that.

The manuscript was originally completed in May, 1923, and the general form of the book corresponds to the aspect of the subject at that time. Brief statements on the more important advances which have since been made have been inserted in the proofs—sometimes in the main body of the text and sometimes in footnotes. The book may therefore be regarded, in respect to its subject-matter, as up to date. The only considerable omission is an account of a very recent paper by Eddington "On the Relation between the Masses and Luminosities of the Stars" (*Monthly Notices of the Royal Astronomical Society*, Volume LXXXIV., p. 308, March, 1924). The possible implications of this paper are so revolutionary in character that it is not possible to take account of them without considerably modifying the whole ground-work on which the book is constructed. It may be that the book will mark an epoch in astrophysical theory—characterised by the ascendancy of the giant and dwarf

theory of stellar evolution. But it is too early yet for any definite conclusion on the matter.

The author's thanks are due, and are gladly accorded, to all who have so promptly and generously responded to his requests for illustrations. The kindness which he has met with in this matter has been of the greatest encouragement. Particular acknowledgment should be made of the valuable assistance given by Mr. W. Bradfield in the preparation of many of the illustrations for the Press. It is impossible to mention individually the various books from which, in addition to original papers, help has been drawn : no available source of information has been neglected. Especially, however, the author wishes to record his indebtedness to Professor A. Fowler, F.R.S., Yarrow Research Professor of the Royal Society, whose time and knowledge have always been freely offered, and whose encouragement and kindly criticism have been of the greatest help. If the author can succeed in passing on to others some of the inspiration he has received from Professor Fowler, he will be content.

H. D.

Imperial College of
Science and Technology.
May, 1924.

PREFACE TO THE SECOND EDITION

Since this book was first published the science of Astrophysics has undergone a remarkable development. Not only has there been an exceptionally rapid growth of a normal kind through the acquisition of new knowledge, but at the same time, as was foreshadowed in the original preface, there has been instituted a reformation of the fundamental ideas of the subject which may perhaps best be described as a metamorphosis. The purpose of the book being the presentation of the various departments of astrophysical research as a unified whole (so far as the essentially fragmentary character of existing knowledge makes such a thing possible), it was intended, if a second edition were called for, to modify the structure of the book in order to represent as faithfully as possible the new aspect of the subject. For example, in the first edition the theory of stellar evolution, being a deduction from some observations and a guide to the interpretation of others, occupied a central position. To-day it is no longer a guide, but, on the other hand, in its very unsettled state, embodies a greater wealth of data. It would therefore more appropriately find a place at the end. It so happens, however, that the demand for the book has exceeded expectations, and the first edition has been exhausted while the metamorphosis referred to above is still in active operation. In these circumstances it has been thought inadvisable to attempt, at the present time, a reconstruction which in a few months might be almost as much in need of reform as its predecessor. The present edition, therefore, except for the correction of a few typographical and other minor errors, is identical with the former one.

The desirability of reissuing the book as quickly as possible prohibits a number of alterations suggested by various critics and by personal reflection and experience. This I greatly regret, and I take this opportunity of sincerely thanking those reviewers and readers who have been kind enough to suggest such alterations. If, as the present demand

seems to indicate, a further edition becomes necessary, I hope a complete revision will be possible, in which full advantage will be taken of their suggestions. The book has been used to a greater extent than I anticipated by working astronomers, and it therefore seems desirable at the same time to include references to original papers, for which many readers have asked. In the meantime the publication of Col. Stratton's book on "Astronomical Physics" meets all requirements in this respect.

With regard to the main purpose of the book—namely, to inform the general educated public of the progress and present state of Astrophysics—the numerous letters I have received are an assurance that the method of treatment of the subject-matter is fundamentally sound. It has often seemed to me that in many popular accounts of scientific matters, insufficient discrimination has been made between the educated, non-scientific man and the child. Too much reliance has been placed on simple illustrations and analogies, with the result that if a conception suggests no such illustration or analogy, or only a very imperfect one, it has frequently been passed over as being too abstruse for the uninitiated. In this book, analogies are almost non-existent. The method of exposition has been direct description, and I think it is very significant that the critics who have suggested that the book is too advanced for the general reader have invariably been scientific men already fully conversant with the subject. The "general educated reader," on every occasion on which to my knowledge he has expressed himself, whether in review or in letter, has declared the treatment to be well within his comprehension. This leaves no room for doubt, I think, that when the terms employed are clearly defined and the sentences used are unambiguous, the mental grasp of the educated layman is not inferior to that of the scientist.

It would be scarcely proper to pass over the progress of the last few years in complete silence, and the following brief account of the more important changes which have taken place during that time is therefore given. The reader will easily perceive that it is not merely supplementary to the main text, but inevitably involves considerable modification, which he will be able to some extent to make for himself. Very little change of detail is required to transform

a cloud from the likeness of a weasel into that of a whale. The changes here indicated are of that transforming character.

It has been found that the absolute brightness of a star depends only on the mass. This relation was formerly understood to hold only for giant stars, but it now appears that it is universal. Consequently, the earlier distinction between giant and dwarf stars (that the former were in the state of a perfect gas and the latter were not) no longer exists. The older explanation of the turning-point in Russell's diagram (see p. 152) therefore fails, and further, since a star must maintain a constant luminosity so long as its mass remains constant, we can no longer believe that a star of constant mass can be bright when young and faint when old, as Russell's original theory required. That theory evidently needs radical modification, the character of which is not yet agreed upon, but it is becoming probable that the required change in its outward form is not so great as was at first suspected. The modification, in fact, seems to be the introduction of an active principle into what was formerly a skeleton. There seems to be no reasonable escape from the conclusion that the source of a star's energy lies in the mass of the star itself, and that the radiation emitted is mainly the product of the mutual annihilation of electrons and units of positive electricity (protons). This annihilation causes a steady decrease in the mass of the star, and consequently a steady falling off of brightness, so that a dwarf star was probably at one time a giant, as the older theory required. It is still probable, in fact, that the statistical curve outlined in Russell's diagram (Fig. 8) represents the history of a single star, but we must conceive of the star as radiating itself away as it passes along the curve. In connection with the turning-point, it should be noticed that the spectral type plotted in the diagram characterises the *atmospheric* temperature of the star, and its variations do not necessarily represent those of the internal temperatures, owing to changes of opacity. The lifetime of a star as a luminous body, according to the new view, is of the order of 10^{14} years.

Opinions are at variance with regard to the mechanism by which the annihilation of matter takes place. According to one view, the rate of annihilation is uninfluenced by variations of physical conditions in the stars; according to another it depends only on those conditions. Supporters of the former view assume the existence of stellar elements of

higher atomic weight than those known on the earth. Supporters of the latter view are at liberty to make a similar assumption, but are at present under no necessity to do so. The existing observational data appear to be insufficient to provide a final decision between the opposing views.

The possibility of dwarf stars, with their high densities, behaving as perfect gases arises from the fact that their atoms are largely dissociated into free electrons and nuclei, which require far less space than the orbits in normal atoms. Consequently, much closer packing is possible, and in fact a few stars, known as "white dwarfs" (absolutely faint stars of early spectral type), are known for which the mean density appears to be of the order of tens of thousands (water=1). There is definite evidence of such high density for one of them—the companion of Sirius. These stars may be very numerous, but their extreme faintness (arising from small surface area, not from small surface brightness) prevents the detection of all but the nearest ones.

The "cloudy layer" theory of the Sun's photosphere has now been almost abandoned. The Sun is generally regarded as being gaseous throughout—behaving, in fact, as a whole, like a perfect gas. The analysis of solar and stellar atmospheres has been considerably advanced, but a short summary of the progress made in this branch of the subject is impossible.

The problem of Cepheid variables has received a great deal of attention, and the bulk of opinion is divided between the pulsation theory and a new theory, first proposed by Jeans, which regards these variables as representing a stage in the process of fission of a star. According to the latter view, a Cepheid is looked upon as a double star of which the components are incompletely separated, the system having both a rotation and an oscillation.

New light has been thrown on the problem of the character of spiral nebulæ by Hubble's discovery of Cepheid variables in several of those objects. Using the period-luminosity relation, the distances can be determined (see p. 340), and the values found are of the order of 250,000 parsecs (about 800,000 light-years). The spirals so far dealt with are believed to be among the nearest to our system. Furthermore, some of the outlying regions of these nebulæ have been resolved by the 100-inch telescope at Mount Wilson into multitudes of stars. Strong support is thus

given to the "island universe" hypothesis. If the newly-determined distances are correct, the rotation periods given by van Maanen (p. 335) indicate improbably high linear velocities, and there is a strong likelihood that van Maanen's measurements are affected by some undiscovered systematic error.

As a result of these advances it will be obvious that the general aspect of the subject has greatly changed; the last two chapters of the book, in particular, require complete recasting. It need scarcely be said, however, that facts do not change. I hope and believe that the distinction between fact and speculation was sufficiently made in the original text, and on that ground feel justified in submitting the book again to the public in its original form.

H. D.

Imperial College of
Science and Technology,
March, 1927.

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INTRODUCTION

We are living in an age when the boldest dreams of antiquity are being realised. The highest peaks are being scaled, the depths of oceans sounded, the farthest lands and least accessible places have been reached. Not content with conquering the land and sea, the men of to-day soar into the skies, giving practical form to the wings of Icarus. The transmutation of matter—at the same time the aim and the inspiration of centuries of chemical research—is accomplished spontaneously before our eyes. If we have yet no elixir of life, we are at least not without those who claim scientific evidence for renewal of youth. More remarkable still, modern inquiry halts not even at the gates of death. Persephone, if not commonplace, is believed by many to be anything but unique. Apparently there is nothing too fantastic or too wonderful to be true.

These observations have, indeed, become trite. We are beginning to understand that what man can imagine, Nature can achieve. But what is not so generally recognised is that there is one science in which the facts not merely equal anticipations, not merely transcend them, but are so astounding that, until they were discovered, they had no counterpart in imagination at all. Astrophysics—the study of the physical and chemical nature of the stars—is the realisation of no ancient aspiration. When the Hebrew Lord boasted his greatness to Job, the only authority he claimed over the stars was concerned with their movement—and their apparent movement at that. "Canst thou bring forth Mazzaroth in his season? Or canst thou guide Arcturus with his sons?" That the stars were other than points was not conceived. The wings of the imagination, like those of Icarus, fell away in face of the Sun. Even Astrology, so careful to endow the planets with souls, failed to realise that they might have bodies. Astrophysics, perhaps alone of the sciences, seems too wonderful to be fictitious.

If we attempt to define precisely what we mean by Astrophysics, in order to indicate the scope of this book, we are faced with a great difficulty. Long years ago, when the

first physicist built his primitive laboratory and the first astronomer turned his eyes skyward, there seemed little likelihood that their successors would ever compare notes with mutual profit. In the centuries that have followed, physics and astronomy have grown and extended their boundaries in all directions, until at last there is a large region in which they overlap. That region is the territory of Astrophysics. But from its very nature it is impossible to explore such an area without being familiar with the domains of Physics on the one side and Astronomy on the other. A river delta is not to be understood unless both land and sea are taken into account. The complete study of Astrophysics would require a thorough treatment of the two older sciences as an indispensable preliminary. This is obviously beyond the scope of the present volume: nor would it be justifiable to assume that the reader is acquainted with the finer details of Physics and Astronomy. The only way out of the difficulty, then, is for us to restrict ourselves, as far as possible, to the common ground of Astrophysics in the strict sense of the word, making whatever physical and astronomical explanations may be necessary as occasion arises. Fortunately for our purpose, in the present state of the science it seems possible to do this; though it must be understood that future progress may become indebted to principles of which we have here been able to make no mention.

But now, how are the boundaries of this restricted region to be recognised? They are not fixed for all time. Year by year—day by day, almost, so rapid is modern progress—they are being pushed farther and farther back on either side. Before we define too rigidly our field of inquiry, let us take a general view of the material it contains and the means of research at our disposal.

We are the inhabitants of one of the smaller satellites of a rather small star. The system to which we belong, though astronomically a very minute affair, is enormously large compared with ordinary terrestrial standards of measurement. Round our primary—a Sun, with a diameter of nearly a million miles—there circulate eight major planets,[1] and a multitude of minor ones. Most of the major planets claim one or more satellites, and the system possesses also a number

[1] In order of increasing distance from the Sun, their names are Mercury, Venus, The Earth, Mars, Jupiter, Saturn, Uranus, Neptune.

PLATE II.

Photo: Harvard College Observatory.

Star Cluster ω *Centauri.*

To face page XXIII.

of comets and meteors. Add to this a quantity of fine dust and possibly occasional small bodies which we may capture in our journey through space, and you have the Solar System—a plane assemblage of bodies demanding, for the full exhibition of its movements, an approximately circular area having a diameter of something like six thousand million miles. We must conceive of this as an isolated system, travelling ceaselessly through a space very sparsely populated by other suns which we call stars. At the distance of the nearest star, the whole system would appear as a point of light, not so bright as many of the stars which are now visible to us.

There is evidence that the stars which we may hope to detect with modern instruments are to be counted in hundreds of millions. They vary over more or less wide ranges in size, luminosity, colour, mass, density—indeed, probably in every characteristic of which modern research is able to take account. The brightness of many of the stars is variable, showing fluctuations with more or less definite periods. Occasionally a new star, or *nova*, appears, grows to great brilliance in a few days, and then gradually fades away. Some of the stars, like our Sun, are single luminous bodies, possibly, though not certainly, accompanied by planets, and forming the nuclei of other solar systems. A very large fraction of the stars, however, are complex bodies, consisting of two or more components revolving round a common centre of gravity. The components of multiple stars are not necessarily identical in any respect. In general, they appear to differ from one another as widely as do stars in different parts of the sky. One component may be exceedingly bright, and the other so faint as to send us no perceptible light at all; and differences in other characteristics may be just as marked. In Plate XXXI. a few of these bodies are represented as they are seen through the telescope, though, as we shall see later, there are many which cannot be recognised as multiple by the telescope alone.

In some parts of the sky, "globular" clusters of stars appear, the members of which seem to have a closer relation with one another than they have with the other bodies in the Universe. Such a cluster is represented in Plate II. In general, these bodies are at immense distances from us, and the apparent compactness of the mass as a whole must not by itself lead us to conclude that the individual stars in a cluster are, on the average, much closer together than are the

stars in the neighbourhood of our own Sun. There is evidence also that some of the less distant stars form similar clusters—"open" clusters, as they are called (see Plate III.)—but, partly owing to their relative nearness to us, they appear much more widely dispersed. Again, some probably similar systems are so near that only the common quality of the movements of their stars reveals their true character. It seems that the unifying agency in these nearer systems (the "moving" clusters), is to be sought in a common origin of the constituent stars, and not in any existing centre of attraction. Unlike the members of binary and multiple stars, their components do not sensibly influence one another's motion. But the existence of star clusters has certainly some significance for the astrophysicist, and must be taken into account in any theory which seeks to explain the origin or development of the Universe.

Clusters, or rather clouds, of stars of a less regular form make up a great part of the Milky Way, or Galaxy, which appears as a faintly luminous belt encircling the sky (see Plate XLV.). There is evidence that the plane in which it lies—the "galactic plane"—is of fundamental importance in determining the structure of the visible Universe. As well as the star clouds, the Milky Way shows large, irregular nebulous areas, and in many places dark lanes and rifts appear, as if the light of the background were hidden by interposed dark material.

Finally, Astrophysics must take conspicuous account of the nebulæ, which exist in large numbers in various parts of the sky. Unfortunately, the term "nebula" is applied to a variety of objects, some of which are probably quite dissimilar in origin and character. Three classes of nebulæ have a particular interest for us—the diffuse, or green, nebulæ; the spiral, or white, nebulæ; and the planetary nebulæ. The diffuse nebulæ are seen as shapeless areas of misty light, sometimes discontinuous in structure or obscured in parts by dark patches. The great nebula in Orion, illustrated in Plate XXXIV., is a fine example. The space occupied by one of these objects is enormous—probably millions of times greater than the average volume of a star—and the density must be inconceivably small. The spiral nebulæ, first revealed as such by Lord Rosse's great telescope in 1845, show a structure of a very definite kind. From opposite points on the circumference of a globe of light, two arms

PLATE III.

Star Cluster *Messier* 11.

To face page XXIV.

issue and curl round the central mass in coplanar spirals. The arms are not uniform in structure. At irregular intervals along their length condensations of material appear to occur. Spiral nebulæ have until recently been assumed to run into hundreds of thousands in number, but it now appears very doubtful whether the majority have actually the spiral structure. We see them in all directions with respect to the plane of the spirals (see Plate XXXVIII.). It is believed by many astronomers that they lie outside the region occupied by the stars and the Milky Way. The planetary nebulæ are, in appearance, more like the diffuse varieties, but fill a smaller volume and have a globular shape, as if they were nebulæ condensing into stars (Plate XXXVI.).

Such, then, are the data with which we have to deal. Out of this collection of miscellaneous bodies we have to build up a connected system. The nature of each type of body has to be investigated, and its relation to the whole discovered. We have to determine what has been the state of each unit, and of the Universe as a whole, in the past, and what changes are taking place in the present. From this, we are to infer what is likely to happen in the future. Let us see what instruments are available to help us in attacking these problems.

The earliest, and ultimately the most fundamental, instrument of Astrophysics is the eye. Of the five physical senses with which we are endowed, the sense of sight is the only one to which we can turn for a knowledge of the sky—if we except the sensation of warmth which we owe to the Sun alone. The heavens are far beyond the reach of our arms ; they send us no odorous vapours ; to the palate they make no appeal. If there is a music of the spheres, it can never reach our ears. To the eye alone the heavenly bodies appear, as points of light, dwelling apart. It would seem that very little knowledge of the Universe could be gained by the unaided eye. In the strict sense of the words perhaps that is true, but when the evidence of the eye is supplemented by the activity of the brain, applied with untiring zeal and patience, results are obtained which at first seem incredible. The history of Astronomy during the period when the eye was the only optical instrument employed, is one of the most striking monuments to the value of keen perception and deductive reasoning. The men of that period knew, with

considerable accuracy, the apparent positions of the stars and the motions of the Moon and planets. They discovered that the distance of the Sun from the Earth was a variable quantity, and found in what way it varied. The precession of the equinoxes was known before the birth of Christ. Kepler, from the visual observations of Tycho Brahe, deduced his famous laws of planetary motion. The conception of the Solar System which we hold to-day was in principle given us by Copernicus in the sixteenth century. If the great work of Newton—the idea of universal gravitation—did not precede the invention of the telescope, it was in no way dependent on telescopic observations. The dynamics of the Solar System, which have been our guide for more than two hundred years, sprang from observations made by the naked eye. And even to-day when our observatories are equipped with instruments of the greatest refinement, the eye has still its work to do. In the extent of field which it can cover at a single glance, it is without a rival, and on this account it is one of the most certain means of discovering novæ that grow bright enough to come within its reach.

Early in the seventeenth century, an unknown Dutchman discovered that, by looking through two lenses separated by a finite distance, an object could be apparently magnified; and, in so doing, he invented the telescope. Immediately the domain of Astronomy grew larger. The planets were seen as discs. Their satellites were visible, and could be seen in actual revolution round the primaries. Saturn was discovered to possess a unique system of rings. But, infinitely disappointing as it must have been to the early observers, the stars remained mere points of light. It is rather curious that the popular idea of the function of a telescope does not at all correspond with its chief use in Astronomy. A telescope is usually thought of as an instrument which magnifies an object, and brings it apparently nearer to the observer. In reality, this property of the telescope is applied practically only in the observation of our own Solar System. The stars and nebulæ, and the Milky Way, are far too distant from us for the telescope to magnify them sensibly. In stellar Astronomy, the great advantage of the telescope is its power of gathering light, and thereby making visible objects which are too faint to be seen by the eye alone. The large telescopes of the present day collect thousands of times as much light from each object as the naked eye could do. As a result of this,

the invention of the telescope led to the discovery that the sky was far richer in stars that it had previously appeared to be. Star clusters and nebulæ were discovered, though it was not always possible to distinguish between them. The existence of double and multiple stars was detected, and Newtonian dynamics was thereby extended to the stars. With improved methods of mounting and adjusting the telescope, the distances of the nearer stars could be measured, and a large amount of information with regard to their movements was made available. The telescope is, in fact, a huge eye—or, rather, the refracting apparatus of such an eye. The information it gives us about the heavenly bodies is enormous, but it is all of one kind. It could all be obtained by the eye alone if we could greatly lessen the distance between ourselves and the bodies which it is used to perceive.

A decided forward movement was made in Astronomy when Sir William Herschel brought the photometer into its service. The photometer is an instrument by which the relative brightnesses of two sources of light can be determined. It is possible, of course, to compare only the apparent brightnesses of bodies directly by the photometer, and these will differ from the true brightnesses if the bodies are at different distances from the instrument. Nevertheless, the actual luminosities can be compared when the distances are known, so that photometry gave us, for the first time, definite knowledge of intrinsic qualities of individual stars. In reality, as well as in appearance, one star was found to differ from another in glory. Later investigations have shown, moreover, that, even from the apparent brightness of the stars as a whole, conclusions of the highest importance can be drawn with regard to the structure of the Universe and the extent of our stellar system.

Astrophysics may be said to have been born with the invention of the spectroscope. The telescope and the photometer, indispensable as they have become, do no more than amplify, and make measurable, knowledge possible to the eye. The spectroscope does more. When a chord of musical notes is struck, the ear detects, not only the combined harmony but the individual notes which go to produce it. The eye does not possess this power; a number of colours mixed together produce one resultant colour, in which the identity of the constituents is lost. Now the spectroscope extends to the eye the analytical properties of the ear. A beam of

light falling on a spectroscope is analysed into the individual colours of which it is composed, and, as a result of the analysis, —in a manner which we hope to explain later—we can gather an enormous amount of knowledge concerning the chemical composition of the stars and nebulæ, their physical constitutions, temperatures, velocities, masses, densities—and possibly a large number of other things as well. The incredible progress of spectroscopy during the last few decades makes it unwise to attempt to impose any limits on the powers of this wonderful instrument. It has shown us how to distinguish at a glance between unresolved star clusters and diffuse nebulæ. It has revealed binary stars whose components are far too close together for the telescope to distinguish them. It has made possible theories of the evolution of the Universe. It has, in short, provided us with a new sense, so that we can treat the stars almost as if we had them in our laboratories, and could do what we liked with them.

If the telescope is an enhanced eye lens, the photographic plate may fairly be regarded as a new retina. The camera serves Astrophysics as much by accelerating the speed of progress as by directing it into new channels. The image on a photographic plate is permanent and transferable. A photograph of the sky or of a spectrum can be reproduced and spread broadcast among astrophysicists, so that what one sows a thousand others may reap. The camera, moreover, opens the way to greatly increased accuracy and observation of details. A photograph can be studied at leisure and as often as one pleases, whereas a visual image is, of course, transient, and possible only when the sky is clear and the body under examination is well situated for observation. These advantages alone would make the camera an instrument of the greatest value. But it can boast two other qualities which the human eye does not possess. In the first place, it can store up light, so that an object too faint to impress the plate in a short time can be photographed by making a long exposure. In this way, thousands of stars, invisible even with the largest telescopes, are detected and studied, and sufficient data have been obtained to give us some idea of the shape and extent of our Universe. Secondly, a photographic plate is sensitive to ultra-violet light, which the eye cannot see. Since many of the most important "colours" in spectra lie in the ultra-violet region, the interpretation of spectra is thereby greatly simplified. On account of these

qualities, practically all modern observations are conducted by photography. Exception must be made of certain special observations, such as those of the surface markings of planets, in which, owing to the vagaries of our atmosphere, the clearest image appears only at isolated moments when the atmosphere is temporarily steady. But for stellar and spectroscopic work, which forms the great bulk of modern Astrophysics, photographic methods are almost invariably employed.

Finally, within the last few years, Astrophysics has found a use for the interferometer. This is an instrument in which two beams of light, which have travelled through slightly different paths from their common source, are made to "interfere" with one another and produce darkness—according to the well-known principles deducible from the wave theory of light. The application of this phenomenon to Astrophysics is not very obvious. Without entering now into details, however, it may be said that it forms the basis of a method of measuring the angular diameters of stars. The telescope, as we have remarked, is powerless to magnify a star appreciably. The rays of light coming from stellar distances are so nearly parallel that, even after refraction or reflection in our largest instruments, they still proceed almost to a single point. Nevertheless, since the stars have finite diameters, the rays from different points in them must converge to some extent, and though their convergence is too small to show a star as a disc of light, it is not too small in all instances to triumph over the extreme delicacy of the interferometer. One more of the seemingly impossible problems has been solved—the direct determination of stellar dimensions.

We have, then, as physical instruments, the naked eye, the telescope, the photometer, the spectroscope, the camera and the interferometer—these six; and we do not belittle the importance of the others in saying that, for wealth and comprehensiveness of data, the greatest of these is the spectroscope. Its supreme value lies in the fact that the spectrum is influenced by almost every change of physical or chemical condition in which we are interested. This has not always been realised—indeed, it is very questionable whether, even now, we know all the influences to which the spectrum is susceptible. The spectroscope was applied, in its infancy, only to chemical analysis in the laboratory. The substance to be examined was vaporised and made luminous, and its light was analysed

by the spectroscope. The apparatus was constructed so that each colour in the analysed light appeared as a fine vertical line, and the whole spectrum (see Plate VI.) was just an assemblage of such lines arranged side by side, each having its own characteristic colour. Under these conditions, the spectrum of any source of light was found to be characteristic of the chemical nature of the source. In this fact lay the possibility of the science of Astrophysics—or, rather, of that aspect of it which may be termed physical astronomy. For the stars send us light, and we can obtain their spectra, and hence possibly draw conclusions as to their constitution. It did not follow, of course, that stellar spectra would necessarily be identical with spectra produced in the laboratory. The Sun and stars might contain totally different elements from our terrestrial ones, and so give different spectrum lines. As a matter of fact, however, when certain initial difficulties as to the interpretation of the spectra had been overcome, it was found that, for the most part, the lines in solar and stellar spectra could be matched by those from familiar elements. With that discovery, Astrophysics became, not only physical astronomy, but astronomical physics as well. The boundaries of physical laboratories receded to the limits of the Universe. In the stars the behaviour of matter could be studied at temperatures and pressures incomparably beyond the attainment of terrestrial appliances. Small wonder that, in the enthusiasm born of such an unprecedented discovery, hopes were entertained which have not been fulfilled. There are limits to the chemical application of the spectroscope. As we shall see later, the spectrum can indicate the composition of only a small part of a heavenly body, and even there the analysis may not be complete. But the partial failure on the chemical side has been more than counterbalanced by the extraordinary wealth of physical data which the spectroscope makes available. It was soon discovered that a substance could give a variety of spectra, each chemically characteristic of its source. The variation is due entirely to physical causes, among which change of temperature is probably the most effective. Hence arose a method of estimating stellar temperatures, and later, from the distribution of light intensity along the spectrum, it was found possible to verify the results obtained. Further, exact measurements of the positions of lines in a spectrum showed that the lines could be displaced or split into

components, in a perfectly definite way, by a number of agencies, such as magnetic and electric fields, mechanical pressure, velocity of the source towards or away from the observer : and each of these effects could be used as a measure of its cause. The relative intensities of particular lines in its spectrum was found to be a function of the total amount of light emitted by a star ; hence stellar brightnesses became known, and, consequently, the distances of thousands of stars, irrespective of how far away they might be, provided they sent enough light to the Earth to produce a spectrum. There are suggestions—though they have not yet been verified to the satisfaction of every one—that the positions of spectrum lines may be dependent on a property of the gravitational field in which they are produced. If this is so, the spectrum may in time give a direct measure of stellar masses. Continuously, as time goes on, the spectrum is giving more and more evidence of its apparently limitless possibilities. It is as though a star throws the whole secret history of its being into its spectrum, and we have only to learn how to read it aright in order to solve the most abstruse problems of the physical Universe.

What, now, shall we take as a definition of Astrophysics ? We should give a fair indication of its field if we said that it consists of all the information obtained from the application of the spectroscope to the study of the heavenly bodies. But if we so limit ourselves, we cannot describe that field in sufficient detail. We cannot altogether dispense with the contributions of the other instruments we have described. There is one quality of almost all the heavenly bodies we examine : they send us light. There is one common characteristic of all astrophysical instruments : they investigate the properties of light, either in quantity or in kind. Shall we say, then, that Astrophysics is the study of the light sent to us by the celestial bodies, and the problems with regard to the constitution and inter-relations of those bodies to which we are thereby led ? On the whole, this seems to be the most satisfactory definition that we can give. But for our present purpose we shall find it necessary to depart from it somewhat. In its general form, it includes the study of comets, for they emit light of their own. We leave these bodies, however, as they can more appropriately be dealt with in a future volume of this series. Furthermore, it will be desirable

to discuss the bearing of modern dynamics on the problems of Astrophysics, and this cannot be said to fall within the scope of our definition. Nevertheless, with these exceptions, that definition accurately describes the field which we are about to explore.

The problems to which the study of our subject leads us are as far-reaching as is possible to a purely physical science. They may be summarised as nothing less than the problem of the distribution of matter and energy in time and space. It may well be thought that such a study is too vast for the present state of our intellectual capabilities and attainments. We experiment in the laboratories of Earth. We watch the changes of matter and energy in a tiny speck of space inconceivably small compared with the vast dimensions of the Universe. Can we dare to apply the laws which, subject to the errors inherent in all human operations, are found to hold in our microcosm, to a space which may be infinite in extent ? Perhaps not : but we must bear in mind that we are essentially inquirers. We are not legislating for the Universe ; we are, humbly and reverently, asking what the Universe has to say to us. When we find certain laws operating in our own little home of Earth, we ask if those laws hold also in the larger world outside. We test our observations by them, and if we find consistency, our trust in them deepens. If there is no consistency, we are not, or should not be, disheartened : we are learning more of the ways of Nature than the Earth could tell us. It is only by placing an implicit trust in the limited generalisations which we have learned to make that we can hope to discover where they are false or incomplete.

When the final account of the physical world comes to be written, it is indeed doubtful how important a place will be occupied by the present body of science. We study the material part of the Universe : the evolution with which we are concerned is the evolution of the stars. And there is, no doubt, great justification for this. Matter forces itself on our attention ; it seems to be everywhere. We look into a sky thick with stars, and we are awed by the multitude of unimaginably massive bodies by which we are surrounded. But, actually, the volume of matter around us is infinitesimal compared with the space in which it moves. To a much higher degree of approximation than we usually employ, we can say that space is empty. Within a radius of a hundred

million million miles we are able to detect 24 stars. If we suppose there are also an equal number of very faint stars, whose distances have not been measured, we obtain 48—or 50 in round numbers. Let us suppose that, on the average, they are each ten times as massive as our Solar System—probably a large over-estimate—then the total amount of matter in a sphere of 100,000,000,000,000 miles radius will be 10^{30} tons, giving an average density of 3×10^{-21} lbs. per cubic foot, or 5×10^{-23} grams per cubic centimetre. How extremely small this is may perhaps be realised when we say that, if the stars gave no light and exerted no gravitational attraction, a being free to wander in space would have as much chance of finding a star as we should have of finding a 2-oz. brass weight in all the oceans of the Earth. And the space we have chosen is in that part of our stellar system which is perhaps the most closely packed with stars! For some reason which we do not yet know, Nature has chosen to concentrate all her matter and the greater part of her energy in tiny points of her boundless space. We study these points because their characteristics are most susceptible to our methods of investigation. But is it not possible, and even probable, that the deeper secrets of Nature are to be found in the great volume of unoccupied space? Biology—which, in the multitude of its individual data and the recognition of progressive evolution, bears a strong resemblance to Astrophysics—has been forced to admit environment as a leading agent in producing its phenomena. May not the time come when Astrophysics will have to do the same? To these transcendental questions we can at present give no answer.

It will have been gathered, we hope, that modern Astrophysics has not a compact, well-rounded tale to tell. We cannot present a comprehensive system of the Universe, arranged in logical order, with each chapter leading inevitably to the next, and with an explanation of every fact and an answer to every objection. It is an incomplete, fragmentary story that is to be unfolded. The band of inquirers moves gradually over the field, scaling a peak here, entering a valley there, but never seeing far in advance. While the march goes on, we pause and look round at the country that has been gained. We sketch it as it appears from the point we have reached, and, looking forward, try to guess what lies beyond. It is not to be expected that our plan will

be complete. The promise of one road will be contradicted by the revelations of another, and even during our survey the advance guard will have reached new standpoints. We do not claim to present ultimate truth; we try only to give the most comprehensive view we can obtain from our present position.

This book is divided into four parts. In the first, the principles of spectroscopy are explained, in so far as they can be applied to the heavenly bodies. It is impossible to understand Astrophysics without a groundwork of this kind. In the second part, we deal with the various physical features of the stars—their masses, velocities, temperatures, etc.—considering each feature in relation to the spectral types. The conclusions that have been drawn from these considerations, with regard to stellar evolution and the distribution of the stars, are then set out. In the third part, the more detailed characteristics of the different types of celestial body are passed in review; and, finally, we consider how far the facts that have been discovered can be co-ordinated into a consistent theory of the structure, history, and destiny of the Universe.

Section I
SPECTROSCOPY

CHAPTER I

THE SPECTROSCOPE, THE SPECTRUM, AND THE ATOM

SPECTROSCOPY is the analytical study of the energy radiated by matter.

There is a curious difference in the behaviour of the two great physical entities, matter and energy. Matter has a natural tendency to condense: each material body attracts other bodies towards itself, and shrinks into the smallest possible space. If gravitation is the predominant force in the Universe, and if, at any one instant, all the matter in space were relatively at rest, the ultimate state of the physical world would be certain: there would be one single body of maximum density—the eternal, subjectless monarch of boundless space. It must be confessed, however, that there is great uncertainty about the postulates. Energy, on the other hand, is predestined to dissipation. There is no receptacle of energy—with the possible exception of certain material atoms and their parts—in which there is no leak. In the ultimate conditions we have just imagined, the extra-atomic energy of the Universe would be diffused uniformly through space.

The necessary consequences of this state of things is that our knowledge of distant bodies is to be sought in their energy rather than in their matter. A star is conservative of its material frame: its energy it sends broadcast into space. Space is filled with the radiated energy of bodies, a part of which we call "light." Century after century the ceaseless messages travel to and fro—the gifts of the stars to their brothers in the firmament. We look at the Milky Way, and our eye receives the energy sent out tens, perhaps hundreds, of thousands of years ago. We see the Galaxy as it was in the childhood of mankind. Throughout the vast interval the light has travelled on, year after year, age after age, at one unvarying speed—186,000 miles per second. From the beginning of physical history this has been going on; every portion of space has held at every instant a supply of flowing

radiant energy, coming from all directions and proceeding outwards and onwards to infinity. Herein lies the possibility of Astrophysics. We are cut off from the material bodies of the stars by the impenetrable barrier of space: nothing can sever us from the immediate portion of the barrier itself. Our prison walls hold the key to their own doors.

The problem before us, in its broadest aspect, is this: Given a minute region of energy-filled space in our immediate neighbourhood, to deduce the extent, configuration, and history of the physical Universe. To see the world in a grain of space; to hold eternity in the glance of an eye—that is the problem of Astrophysics. To make the matter clear, let us at the outset form a hypothesis with regard to the nature of radiant energy. We will assume that space is filled with a continuous, all-permeating substance, which we will call "the Ether." We will conceive of the Ether as a sort of ocean, boundless in all directions, surrounding and interpenetrating the pores of all material bodies. What we have so far spoken of as "space," we shall henceforth call, indiscriminately, "space" or "the Ether." Radiant energy, which includes light, we shall regard as waves or undulations in the Ether, issuing from all bodies in all directions, and absorbed, reflected, or transmitted by the bodies on which it falls. Into the properties of the Ether, and the many difficulties which our conception of radiant energy arouses, we shall not enter. Of late years, the very existence of the Ether has been questioned: with that also we shall not concern ourselves. For our purpose we require, not a statement of eternal truth on this matter, but a mental picture of what is going on—a picture which, whether true or not, will not mislead us in a review of the knowledge which astrophysicists have so far been able to accumulate. We shall find the conception which we have indicated satisfactory in this regard.

The general problem can now be restated. Given the Ether in immediate contact with our sense-organs and instruments, with its resultant energy content arising from the multitudinous waves traversing it, we are to determine the distribution of matter and energy in time and space. There is only one way of dealing with such a problem. We must analyse the radiation we receive, so as to determine, first, the directions from which its constituent parts come, and second, what each component train of waves can tell us about the

nature of its source. The first step of this twofold analysis is performed for us by the eye, and we are so accustomed to it that we are apt to overlook the fact that it might very well have been otherwise. When we look into the sky and say: "Yonder is Betelgeuse; close by are Rigel and Bellatrix, and there is the great Orion nebula," we are not stating original data for observation, but the result of a process of analysis. The radiation received through the pupil of the eye is composite: the eye dissects it into a number of specific beams, so that we can say: this portion comes from Betelgeuse; this, from Rigel; this, from Bellatrix; and this, from the great nebula. Had the eye been merely a retina, we should no more have been able to "see" any object than we can trace the path of a billiard ball from the final direction of its motion. In each case it is a resultant effect that is immediately presented to us. The crystalline lens of the eye is the primary analyser of light. It sorts the medley of waves that falls on it into an ordered array, and so makes possible the first step towards the solution of the fundamental problem.

For the second step, the eye alone is insufficient. Its analytical function is concerned chiefly with direction. It distinguishes, in its datum of energy, a number of separate beams, but it tells us very little about the peculiar characteristics of the individual beams. It does tell us something, however; enough to suggest the possibility of a much more thorough analysis. Apart from differences of brightness—which we can readily account for as arising from an unequal distribution of energy among the different beams—we can detect differences of colour. Any one can distinguish the red glow of Antares from the ice-blue light of Vega. Evidently, in colour we have an indication that radiant energy is not all of one kind, and the possibility arises that the colour-quality of radiation may have an intimate connection with the physical condition of the body from which the radiation issues. This is, in fact, found to be so: colour analysis is another name for the operations of spectroscopy.

To prepare for this new development, we must make our conception of radiant energy more precise. The waves which we postulate must be supposed to be capable of variation in some particular quality, so as to account for the differences in colour we perceive. There are three characteristics of wave-motion which are, *prima facie*, available for our choice. There is, first, the "velocity" of the waves, *i.e.*, the distance through

which any one wave travels in unit time; secondly, there is the "wave-length," *i.e.* the distance between two successive crests in the wave train; and, finally, the "frequency," or the number of waves passing a given point in the Ether in unit time. To the variation of one or more of these characteristics we must attribute the colour differences found in light from different bodies. There are conclusive reasons for rejecting velocity as the variable factor: we are bound to assume that, in free space, all waves travel with exactly the same speed. Colour must, therefore, be accounted a function of wave-length or frequency. As a matter of fact, the uniformity of velocity for all waves makes these two qualities dependent on one another. From the definitions it will be clear that the velocity of a train of waves is the product of the wave-length and the frequency. Hence, constancy of velocity implies that the wave-length is inversely proportional to the frequency, and a change of one entails a corresponding change of the other. We are thus led to the conclusion that, if our conception of radiant energy is to remain a possible one, we must suppose the Ether to be capable of transmitting waves of different lengths (and correspondingly different frequencies) with the same velocity. For example, red light is said to have a greater wave-length than blue light, and, accordingly, a smaller frequency.

When the wave-lengths and frequencies of different kinds of radiation are determined, it is found that their values are, in general, so different from the orders of magnitude we are accustomed to measure, that new units must be employed in order that the figures dealt with shall be of reasonable dimensions. The wave-length of a particular green colour, for example, is 0.0000005 of a metre. Numbers of this order are inconvenient for ordinary use. A unit is therefore employed whose value is, as nearly as possible, 10^{-10} metre: it is known as the "tenth-metre" or "angstrom unit (A.U.)." The radiation just mentioned would accordingly have a wave-length of 5000 A.U.[1] The frequency of this radiation, on the other hand, is inconveniently large in ordinary units. Dividing the wave-length (in metres) into the velocity of light (3×10^8 metres per second), we find a frequency of six hundred million million waves per second. To avoid the use of such enormous figures as this, frequencies are usually

[1] A unit of wave-length less frequently used is the *micron*, which is equal to 10^{-6} metre; thus, 1 A.U. $= 10^{-4}$ micron.

indicated, not by the number of waves sent out per second, but by the number contained in one centimetre of the wave-train. This quantity, which is known as the " wave-number," is evidently to be obtained by dividing the true frequency by the velocity of light, and gives a result, in our particular example, of 20,000. To sum up, then, we say that a certain radiation has a wave-length of 5000 angstrom units (usually written λ5000 A.U.), and a wave-number of 20,000 (or ν20,000). Visible light extends from about λ7600 in the red to λ3900 in the violet, and beyond these limits we have, of course, the infra-red and ultra-violet waves, to which the eye is insensitive.

We are, obviously, not justified in assuming that all the light from a particular source is " pure," *i.e.*, composed of waves of one single wave-length. The red colour of Antares tells us only that the waves of a particular shade of red are predominant in the light from that star. There may be a large number of other waves in the total radiation whose individual colours are masked in the resultant effect. The task of spectroscopy is to separate the colours, and to exhibit them in sequence as a " spectrum " of the source. Two ways of doing this suggest themselves, and both prove to be practicable. Remembering that the radiation to be analysed consists of superposed waves travelling with the same speed in the same direction, it is clear that the separation can be made if we can either accelerate or retard the waves by different amounts depending on their wave-lengths, or, alternatively, divert each type of wave into a path of its own. Modern spectroscopes employ both of these processes.

It will, perhaps, seem strange that, after declaring that all waves are propagated with the same velocity, we should suggest a variation of velocity as a means of separating them. But we must notice that uniformity of velocity holds only in empty space ; that is, in the free Ether. The conditions are different when the Ether is not free but is in the interior of a transparent material body. In these circumstances, the velocity of all the waves is reduced, by an amount which is greater, the shorter the wave-length. Suppose a composite beam of radiation is made to pass through a block of glass cut in the shape of a triangular prism (Fig. 1). The reduction of velocity will declare itself by a bending, or refraction, of the radiation, the shorter waves being bent more than the longer ones. The directions of the different waves in the

glass will therefore depend on the wave-lengths, and, on emerging from the glass, the beam will have been analysed into its components. The separate colours will now proceed in different directions instead of being superposed on one another. One advantage of cutting the glass into a prismatic form is that the rays of light, after passing through it, undergo a double separation, corresponding to refraction at the two surfaces, and issue into the air again with the colours well separated. Fig. 1 shows the paths of the rays in these circumstances.

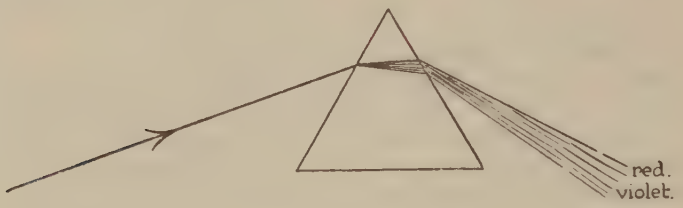


Fig. 1.

The second method of analysis—by which the waves of different lengths are made to pursue different paths without the interposition of a retarding medium—is made possible by the fact of "interference" of light. The beam to be analysed is directed towards a "diffraction grating," which consists essentially of a number of very narrow surfaces (either transparent or of high reflecting power) separated by very narrow absorbing surfaces. In practice, gratings are made by ruling a number of very fine, close, equidistant, parallel straight lines on a clear glass or polished metal surface. As many as fifteen or twenty thousand lines are generally ruled on each inch of the grating. The rulings (which necessarily have a rough, irregular surface) absorb or diffuse the light falling on them, while the clear intermediate strips either transmit or reflect the light, according to the properties of the material on which the grating is ruled. The result in each case is that the light proceeding from the grating consists of a large number of very narrow pencils, issuing in all directions (see Fig. 2). Consider, now, the light going in any one direction. When its multitudinous pencils are collected by the eye, it is found that, except for waves of one particular length, characteristic of the direction chosen, they annul each other, so that the

eye sees, in general, one colour only. If any colour is not present in the *incident* light, then the eye sees nothing at all in the direction corresponding to it. For each direction of the emergent light a surviving wave-length is possible, and,

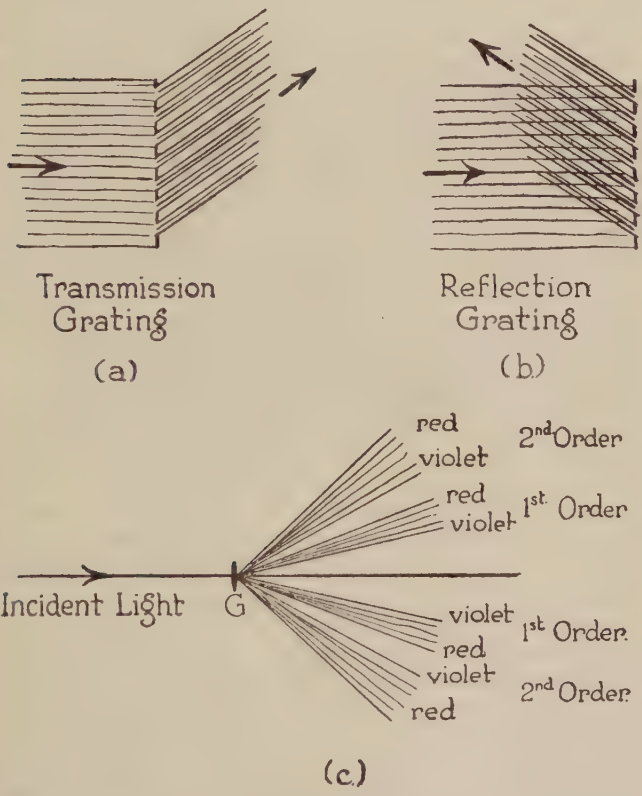


Fig. 2.—Diagrams showing Paths of Rays from Grating.
(*a*) and (*b*) Monochromatic Light (one order only shown).
(*c*) White Light.

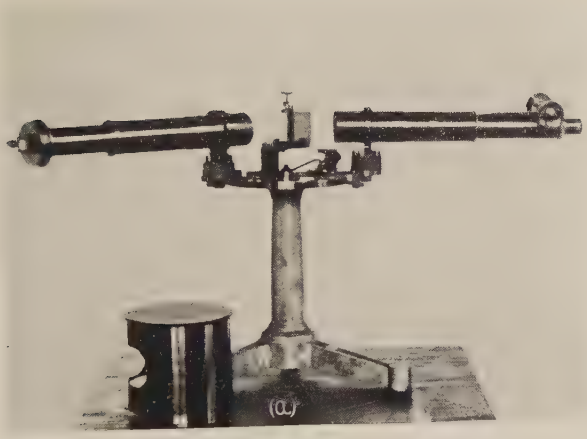
as the direction of view is gradually changed, the light perceived changes continuously from longer to shorter, or shorter to longer waves: in other words, a spectrum is produced. As a matter of fact, more than one spectrum is formed by a grating from the same incident beam. Each wave-length

can proceed in a number of directions without being destroyed, and so can take part in a succession of spectra. In Fig. 2. two spectra are shown, in diagrammatic form, on each side of the line of incidence of the light. They are known as the "first and second order" spectra. The degree of separation —or "dispersion," as it is called—of the different colours, increases with the order of the spectrum, but, as a rule, most of the light goes into the lower orders, leaving the higher ones relatively faint.

The central feature of a spectroscope is, of course, the analyser, whether prism or grating. But, in order to use the instrument to the best advantage, certain accessories are required, which we can describe very briefly. There are three qualities which it is desirable for the spectrum to possess. In the first place, all the rays of light *of any one colour* should leave the analyser in the same direction, otherwise the different colours would overlap one another. This is possible only when the incident radiation is uni-directional. The incident radiation is, therefore, made to pass through a "collimating" lens, so that all the light falling on the analyser shall consist of parallel rays. Secondly, in order that colours whose wave-lengths are very nearly equal shall be separate from one another in the spectrum, each colour is made to appear as a very narrow line (see Plate VI.). For this purpose, the effective "source" of light is not the actual origin of the radiation, but a narrow slit, placed between the luminous body and the collimating lens, and adjusted to be parallel to the refracting edge of the prism or the rulings of the grating. When the illuminated slit, instead of the luminosity itself, is placed at the focus of the collimating lens, it becomes a virtual source, and each colour in the spectrum appears as an image of the slit. Finally, the spectrum should be examined on as large a scale as possible. It is, therefore, seen through a telescope of sufficient aperture to receive all the light which leaves the analyser. A complete prismatic spectroscope, in its simplest form, together with a diagrammatic sketch of the path of the rays, is shown in Plate IV. The arrangement when a grating is used is precisely the same. If the spectrum is to be photographed, the eyepiece of the telescope is replaced by a camera.

Various devices are employed for increasing the amount of separation of the lines in the spectrum. The number of prisms may be increased, or a telescope of large focal length

PLATE IV.



(*a*) One-Prism Spectroscope.

(*b*) Path of Rays.

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may be used. Both of these methods have disadvantages. An increase in the number of prisms involves loss of light by absorption in the prisms and direct reflection from their surfaces: a long telescope—which is usually associated with a collimator of comparable length—makes the instrument very cumbersome. Perhaps the best form of spectroscope (or "spectrograph," as the instrument is called when it is adapted for photography), for work on a large scale, is the arrangement devised by Littrow. The idea of this instrument is to secure the advantages of a long focus telescope in the most compact way. The whole arrangement is enclosed in a rectangular wooden case, represented in Plate V. The light enters by the slit, which is at the virtual focus of the collimating lens, and falls parallel on the prism or grating. In the prismatic form, the light is then reflected by a mirror through the prism again, where it undergoes a second dispersion, and re-enters the lens, which now acts as the telescope object-glass. The spectrum is formed at S, where the photographic plate is placed to receive it. When a grating is employed in the Littrow spectrograph, it must be of the reflection type. The instrument is then just like the one described, with the grating in place of the prism and mirror. It will be seen that the Littrow spectrograph achieves its end by using the same tube and lens as both collimator and telescope: it thereby gains compactness and preserves stability.

There is one form of grating with which neither collimator nor telescope is necessary; namely, the *concave* grating. This is a reflection grating of the ordinary type, except that the rulings are made on a concave spherical surface instead of on a plane. If such a grating is placed tangential to a circle of half its radius, and the slit is placed anywhere on the circumference of the circle, then it can be shown that the various orders of spectra will all lie accurately in focus at various places on the same circumference. No lenses of any kind are required—unless it is thought desirable to use one in order to focus the light, from the original source, on the slit.

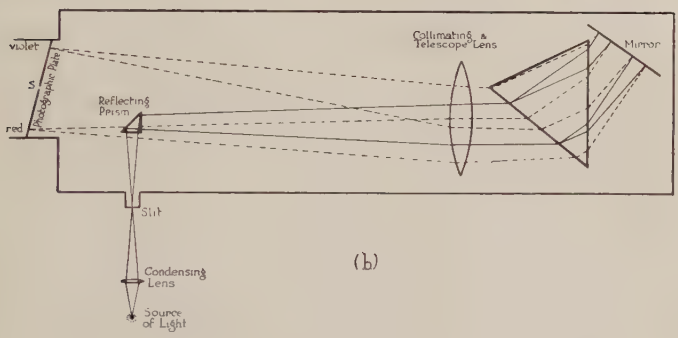
We have given primary attention to the spectroscope, because it is the principal intermediary between the secrets of Nature and our own understanding. On the one side lie the inconceivably distant radiating bodies—for ever beyond our reach: on the other is the spectrum, which we are learning

day by day to interpret. We shall now take a preliminary glance at the spectrum, and afterwards try to form a picture of the radiating mechanism to which it owes its origin.

Every one is familiar with one kind of spectrum, exemplified in the rainbow. Here the spectroscope is the accumulation of water drops forming the clouds, and the luminous body is the Sun. The spectrum, it is true, is by no means so perfect as those which can be produced by the instruments we have just described: although a spectrum of the Sun, it does not present the most characteristic and important features of the true solar spectrum. The colours overlap, there is very little dispersion, and each colour forms an arc of a circle instead of a thin straight line. Nevertheless, the rainbow gives a very good idea of one type of spectrum—the continuous type. A continuous spectrum, produced in the laboratory by a suitable instrument, shows, in the visible region, a straight strip of colour, changing gradually from deep red, through orange, yellow, green, and blue, to the far violet. We regard a spectrum of this kind as one exhibiting light of all wave-lengths from one extreme to the other. Each wave-length is represented by a line, but the lines, being indefinitely close together, merge into one another and give a continuous band of colour. A continuous spectrum may be produced by any glowing solid or liquid body, irrespective of its chemical nature. The glowing filament of an electric lamp, for example, gives an excellent spectrum of this kind.

It will be interesting to follow the course of production of such a spectrum from a body whose physical condition is under our control. Suppose a piece of iron is heated. At first, the eye perceives no change. We know, however, that heat is radiated, and the spectrum assures us that this radiation takes the form of etherial waves, too long to give us the sensation of sight. With suitable means of detection, which we need not consider now, we find that we have the beginnings of a continuous spectrum, in the far infra-red region. If we gradually make the iron hotter, we find from the spectroscope that two things happen. First, the quantity of radiation of every kind emitted increases continuously; second, shorter and shorter waves make their appearance as the heating process goes on. The first of these effects accounts for the increasing amount of heat we receive when we place our hands near the iron. The second explains why, after a time, the iron acquires a dull red colour: it has reached a temperature

PLATE V.



Large Littrow Spectrograph at the Imperial College of Science and Technology.

(*a*) Instrument with end case removed.
(*b*) Diagram of Path of Rays.

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at which it can pour out radiation short enough to stimulate our sense of sight ; or, in other words, the continuous spectrum has now stretched as far as the beginning of the visible region. At a higher temperature, the shorter orange and yellow waves appear, and dominate the red, so that the iron successively assumes those colours. Next, the green and blue waves arrive, but since the red and yellow are still there, the iron does not appear green and blue, but white. Then, at a still higher temperature, we get violet and ultra-violet waves. The spectrum now appears as a regular, uninterrupted band of radiation, of which a small part in the interior is visible as a strip of brilliant colours. We have mentioned iron in this example merely in order to fix our ideas. Any other solid or liquid body would do, provided the temperature is not raised high enough to vaporise it. The continuous spectrum is characteristic, not of a particular substance, but of a particular state of a substance. It is of no use whatever in chemical analysis.

Quite a different type of spectrum is produced by the light from a glowing gas or vapour. Such a source gives, not a continuous range of wave-lengths,[1] but a finite number of discrete waves, each having its own characteristic length. The spectrum, accordingly, appears broken up into a number of lines, of various grades of intensity, with dark spaces between (Plate VI.). Each line is an image of the slit of the spectroscope, in its own particular colour, and its position in the spectrum (which, of course, is dependent on the direction in which its light issues from the prism or grating) gives a measure of the length of the waves which produce it. A line spectrum is characteristic of its luminous source in both chemical and physical qualities. No two chemical elements give the same line spectrum. Since it is possible, by raising the temperature sufficiently, to vaporise every substance known, we have here the essentials of a system of chemical analysis. A single element, however, does not give the same spectrum in all circumstances. It appears, in fact, that almost any variation in the conditions under which luminosity is produced gives rise to a corresponding variation in the spectrum obtained. We shall consider shortly the far-reaching effects of this phenomenon : for the present, we will note merely the differences arising from three standard methods of exciting

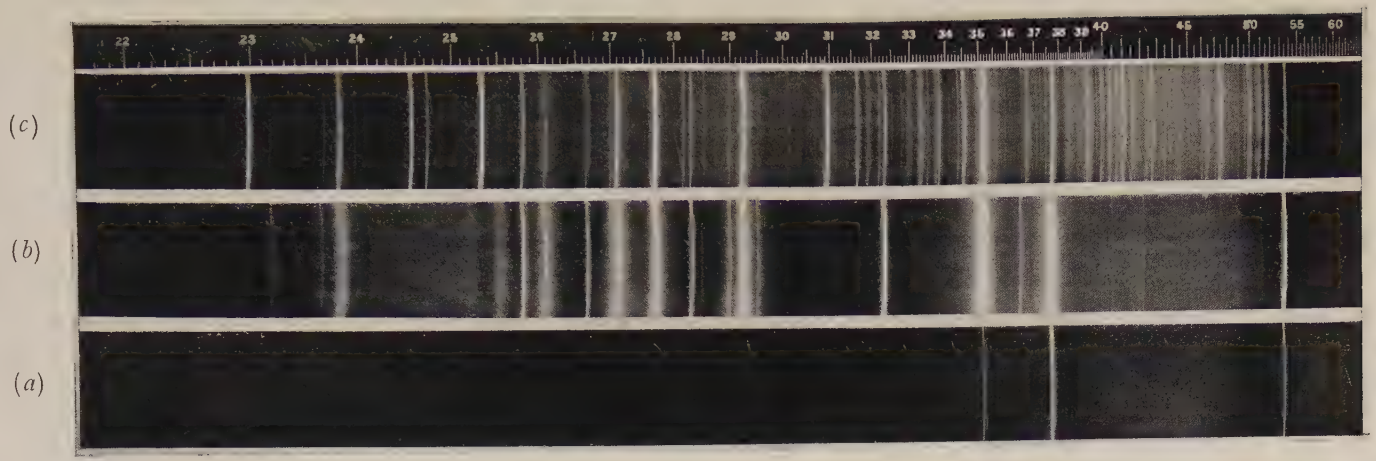
[1] A particular colour in the spectrum is often called a "wave-length."

a substance to radiate its energy. In Plate VI., three spectra of the same element—thallium—are shown. The first, (*a*), is obtained from the light generated when thallium chloride burns in the flame of a Meker burner. The salt is vaporised and dissociated into its elements, thallium and chlorine. The chlorine escapes into the air, and the thallium becomes luminous and produces the spectrum shown. The second photograph, (*b*), shows the spectrum yielded by an electric arc with thallium poles. The third, (*c*), is the spectrum of an electric spark from an induction coil passing between poles of metallic thallium. The scale of numbers at the top indicates the wave-lengths in hundreds of angstrom units.[1]

Spectra produced by these methods are known respectively as "flame," "arc," and "spark" spectra. Certain differences between them will be obvious. The flame spectrum consists of four lines, of which only one, a green line at λ5350, is in the visible region (the colour, of course, is not reproduced in the photograph). In the arc spectrum, these lines appear with a large number of others. The arc spectrum, in fact, generally consists of the flame spectrum with a large number of added lines. The spark spectrum contains yet more additional lines, but it is not represented merely by adding lines to the arc spectrum. Some of the arc lines (*e.g.*, λλ5350, 3229, 2826, 2665), are much weaker in the spark and some (*e.g.*, λλ2843, 2585, 2517), are absent altogether. There is reason to believe that the arc and spark spectra differ from one another in a much more fundamental manner than do the spectra of the flame and the arc. The lines which appear only in the spark, or are strengthened in passing from the arc to the spark, are generally called "enhanced" lines. In Plate VI., there are many conspicuous enhanced lines, *e.g.*, at λλ3092, 2531, 2298. It should be mentioned that a large number (though not all) of the lines in the spark spectrum of wave-length greater than 3100 are due to the gases of the atmosphere which always yield their spectra when a spark is passed in air. We shall see that enhanced lines play a very important part in astrophysical work.

[1] From the point of view of spectroscopic theory it is interesting to note that, while in the flame spectrum the first chief line and satellite of the first member of the diffuse series (λλ3519, 3529) appear in considerable strength, there is no sign of the second chief line at λ2768.

PLATE VI.



Spectrum of Thallium under various conditions.

(*a*) Flame of Meker Burner.
(*b*) Arc between metallic electrodes.
(*c*) Spark between metallic electrodes.

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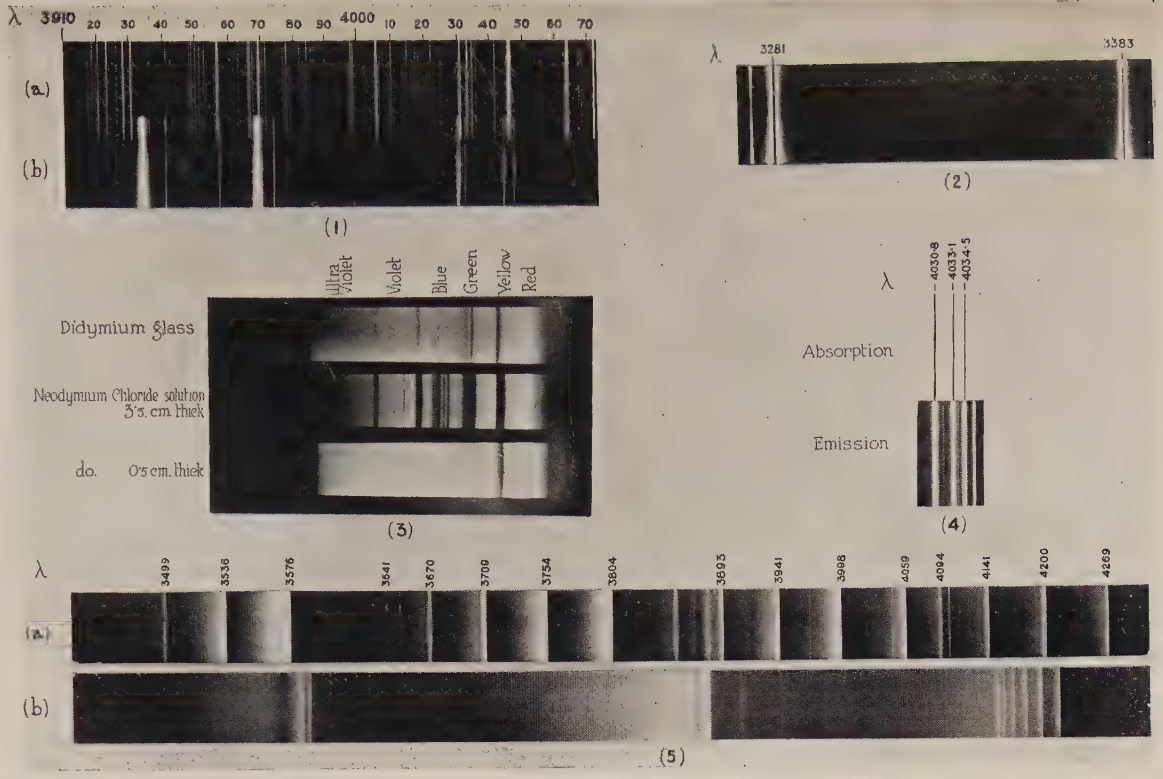
A line spectrum is not the invariable result of analysing the radiation of a glowing gas. Sometimes what are called "bands" are obtained. A band appears at first to be a short strip of continuous spectrum, sometimes sharply defined at one or both edges, and sometimes gradually fading away (Plate VII.) (5). On high magnification, a band is found to consist of a large number of closely-packed lines, arranged with evident regularity, and approaching indefinitely close to one another as a sharp edge or dense central portion is approached. The incandescent vapours of compounds invariably give rise to band spectra, and certain elements also may do so in suitable circumstances.

Continuous, line and band spectra are classed generically as "emission" or "radiation" spectra, because they are obtained from radiation emitted by the body in which they originate. There is another kind of spectrum, however, which is due, not to emission but to absorption. If a transparent, or semi-transparent, substance is placed in the path of a beam of continuous radiation, certain waves may be absorbed by it, and the resulting spectrum will then show dark lines in the places corresponding to those waves, on a continuous background due to the unabsorbed radiation. Plate VII. (3) shows the absorption spectra of didymium glass and neodymium chloride solution. The continuous background was obtained from a Nernst lamp, and the absorbing substances were placed between the lamp and the slit. An absorption spectrum is characteristic of the absorbing substance, just as an emission spectrum is characteristic of the emitting substance.

There is a special type of absorption which is of pre-eminent importance in Astrophysics, inasmuch as it is exemplified in the spectrum of almost every star examined. We have already spoken of the emission spectrum of a glowing vapour. If, now, such a vapour is used as an absorbing substance, and placed in the path of continuous radiation from a *hotter* source, the absorption lines in the resulting spectrum will correspond exactly with the emission lines which the absorbing substance would give if it were acting alone. This phenomenon is shown in Plate VII. (4) for the vapour of Manganese. The upper spectrum shows the absorption by a manganese flame of the light from a much hotter continuous source, while the lower spectrum shows the emission from manganese vapour in an electric arc. The reason why

some of the lines in the lower spectrum are not reversed in the upper one is that the absorbing flame was not hot enough to produce those particular lines, and therefore could not absorb them. A luminous vapour, then, can give its spectrum either as bright emission lines on a dark background, or as dark absorption lines, in the same positions, on a bright background—provided that it is possible to obtain a continuous spectrum from a source of light at a higher temperature than the vapour itself. Subject to this condition, we can get the flame, arc, or spark absorption spectrum of a substance by placing the appropriate form of luminosity in the path of continuous radiation. This phenomenon is sometimes seen in single lines in an arc or spark emission spectrum. Plate VII. (2) shows an example, in which the spectrum was obtained from an arc passing between silver poles. The hot central core of the arc was surrounded by a cooler flame of still luminous silver vapour. The broad, bright lines emitted by the core constituted short lengths of continuous spectrum, and on them are seen the sharp absorption lines produced by the surrounding flame. A line which absorbs its own light in this manner is said to be " reversed." Naturally, flame lines show a much stronger tendency to reversal than others, because they are the chief lines emitted by the cooler vapour surrounding the arc. It may seem strange, in view of Plate VII. (2), to say that, in the absorption line itself, there is a great deal of light, yet this is actually so : the line appears dark mainly by contrast. The exposure given to produce the figure was not long enough for the fainter light to make an impression on the plate ; but, as we shall see later, the photography, by prolonged exposure, of the light in absorption lines has yielded important information with regard to the structure of the Sun's atmosphere.

The facts regarding absorption by luminous gases and vapours were first clearly stated by Bunsen and Kirchhoff in 1859. Up to that time, astronomical absorption spectra had been a mystery : it was not realised that they bore any relation to the emission spectra which were studied in the laboratory. With Bunsen and Kirchhoff's discovery, the possibilities of Astrophysics became unlimited. " What first were guessed as points we now knew stars." The year 1859 is justly celebrated in scientific history as the year in which Darwin gave *The Origin of Species* to the world. The storm of controversy which was aroused by the entry



Emission and Absorption Spectra.

(1) (*a*) Iron, (*b*) a Mixture—Arc Emission Spectra. (2) Silver Arc Emission Spectrum.
(3) Absorption Spectra. (4) Emission and Absorption Spectra—Manganese vapour.
(5) Band Spectra—(*a*) Nitrogen ; (*b*) Cyanogen.

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of the idea of evolution into Biology has perhaps blinded our eyes to the simultaneous discovery—infinitely farther reaching in time and space—by which Bunsen and Kirchhoff made possible a knowledge of the evolution of the physical Universe. In itself a discovery of modest appearance, it yet pointed the way to a path whose terminus recedes farther and farther into the distance as time goes on. Small events sometimes change the face of the world : in the realms of Science, this is nowhere more strikingly exemplified than in the case before us.

To summarise the preceding paragraphs, then, we may broadly divide spectra into two classes—emission and absorption ; either type may be continuous or discontinuous. Discontinuous spectra may consist of lines or bands, or both, and, although always characteristic of their chemical origin, depend also, to a large extent, on the physical state of the body producing them.

The classification and analysis of spectra are the direct aims of the spectroscopist, but he has also an ulterior motive. What is taking place in the radiating body that results in the spectrum we see ? That is the question graven on the heart of modern Spectroscopy and Astrophysics. That it is a question of a fundamental character goes without saying : the very fact that no two elements give the same spectrum suggests—what is indeed the case—that the spectrum arises from the ultimate chemical particles of the luminous body—the atoms or molecules of which it is built up. And just here lies the final significance of the spectrum. Radiation arises from processes occurring within atoms and molecules, so that the study of spectra might be expected to reveal something of the conditions which exist inside those units of matter. Spectroscopy, which began as a disciple of Chemistry, has now become its lord. As a means of analysis, Spectroscopy served the older science faithfully. But chemical analysis is now known to be only an approximation to the final subdivision of matter. To say that this is hydrogen, and that is oxygen, is one thing : to say exactly in what the difference consists is quite another. Chemistry could say only that the atoms of the two substances differed in weight and in the nature of their reactions with other atoms ; and neither fact was able to throw much light on the internal structure of the atoms themselves. But the spectrum speaks of deeper things. So

intimate, indeed, is the connection between the configuration of an atom and the spectrum it yields, that we might almost regard the spectrum as the atom itself projected on a plane, with its essential characteristics represented symbolically by the spectrum lines. The analysis of spectra, which is necessary for their complete interpretation, we shall discuss later, but it seems desirable to give at once a general account of modern ideas of the atom, in order that the reader may form a mental picture of what is happening when radiation is emitted.

The model of the atom which has been of the greatest service in the interpretation of spectra is the conception, partly of Sir Ernest Rutherford, of Cambridge, and partly of Dr. Bohr, of Copenhagen. The atom is represented as something like a miniature solar system. The central nucleus, or "Sun," of the system consists of a quantity of electricity having a net positive charge. Corresponding to the planets, there are a number of revolving "electrons," or unit negative electric charges, just sufficient to balance the positive charge of the nucleus and make the atom, as a whole, electrically neutral. The spatial dimensions of the nucleus are negligible, but its mass is almost equal to that of the whole atom, the mass of the electrons being comparatively of little account. One element is distinguished from another merely by the amount of the charge of the nucleus—and, as a consequence, by the number of revolving electrons and the mass of the atom, since mass, according to modern ideas, depends entirely on electromagnetic condition. Hydrogen, the lightest element known, is believed to have a unit positive charge as the nucleus, and one electron in revolution round it. Helium, the next element in order of weight, has two net positive charges and two revolving electrons; and so on, an increase of one unit in "atomic number," as the number of electrons is called, marking an advance from one element to the next in the periodic table of chemical elements. Now, to understand what happens when radiation takes place, let us consider the simplest atom of all—the atom of hydrogen. It is necessary to suppose that the single electron of hydrogen is not free to move in *any* orbit round the nucleus, but is restricted to one of a number of specified orbits, separated from one another by finite distances. Fig. 3. shows some of the possible orbits of the hydrogen atom. Their radii are proportional to the squares of the natural numbers. In the normal condition

of the atom, when it is radiating no energy, the electron is assumed to revolve in equilibrium in the innermost orbit, obeying laws similar to those which express the motion of the planets round the Sun. Now suppose that, by some agency, the gas is excited to luminosity. It is believed that the electron is pulled away to an outer orbit, and then falls inward again. On its return, it does not necessarily proceed

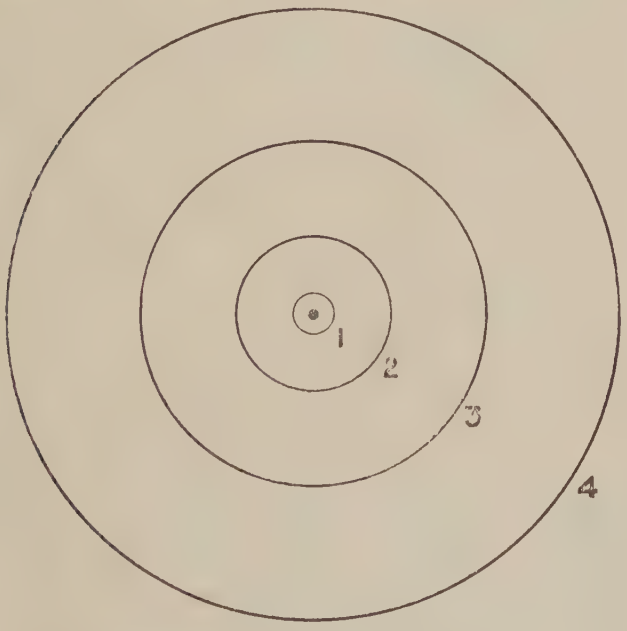


Fig. 3.—Electron Orbits in the Hydrogen Atom.

at once to its previous orbit. It may pause in any one of the others, and revolve there in equilibrium as before—returning to its normal home in easy stages. An atom is thus capable of existing, at least temporarily, in different states, each characterised by the orbit chosen by the electron. Now, in each state, the atom has a certain amount of internal energy, and this amount varies from one state to another. The smaller the orbit in which the electron revolves, the smaller is the amount of energy possessed by the atom, so that, in the normal state, the atom has its minimum energy. The exciting

agency, therefore, in removing the electron to a distant orbit, adds energy to the atom, and when the electron returns the atom must give up some or all of this energy, according to the orbit in which the electron decides to revolve. It is just the energy which the atom thus yields up that is held to account for the radiation. It is radiated in the form of an etherial wave, of a certain definite frequency which is proportional to the amount of the energy. Thus, if the electron is removed from orbit No. 1 to orbit No. 4 (Fig. 3.), and returns to orbit No. 2, one line of the hydrogen spectrum is produced, whose frequency is proportional to the difference of atomic energy corresponding to the states 4 and 2 : when the electron finally returns to orbit No. 1 again, another line is produced, with a frequency similarly proportional to the difference of energy between the states 2 and 1, and so on. Every possible passage from an outer to an inner orbit corresponds to a possible line in the spectrum. Any one atom can, of course, be engaged in radiating only one line at any one instant. The complete spectrum is the sum of the radiations from all the atoms in the luminous gas.

Radiation from the atoms of heavier elements is conceived to take place in essentially the same manner. Here, however, we have more electrons to consider : calcium, for example, has 20 revolving electrons in its atom, and uranium, as many as 92. With all these more complex atoms the mathematical difficulties of calculating the exact radiation, with the completeness attained for hydrogen, have so far proved unsurmountable. A general picture of what takes place is, nevertheless, quite possible. Radiation is attributed to the changes of orbit of the outermost electron in the atom, which revolves in the electric field produced by the remaining electrons and the nucleus. Since this field will vary with the number of electrons and the charge on the nucleus, the atoms of different elements will have different possible orbits, and, therefore, different amounts of energy in corresponding equilibrium positions. Consequently, the spectrum of one element will be different from that of any other, and the possibility of spectrum analysis is immediately accounted for. It should, perhaps, be mentioned that the conceptions involved in this theory of radiation are at variance with the principles of Newtonian dynamics. Of late years, similar anomalies have appeared in other physical phenomena, and thereby hangs a most interesting tale, which it is not our

purpose here to unfold. Suffice it to say that modern investigations seem to leave no doubt that Newtonian mechanics is not the last word on the most elementary movements of matter.

We are now in a position to understand something of the origin of the different types of spectrum we have considered. The continuous spectrum we have seen to be the product of a luminous solid or liquid: how can we account for it in terms of our conception of the atom? The problem does not seem easy at first sight: the discrete character of the electronic orbits seems capable of accounting only for a line spectrum. But we must remember that we have thought of each atom as if it were quite free from external influences. We might compare it to a peal of bells, capable of giving certain notes and no others. If those notes are to be heard separately and distinctly, the bells must have free and uninterrupted play. But the atoms of a solid or liquid body are not in a state to allow of this. They are always well within one another's sphere of influence, so that the radiating atoms suffer violent disturbances, and instead of the sharp, clear lines characteristic of the free atom, we get a confused medley of radiation of all kinds, "like sweet bells jangled, out of tune, and harsh." The result is a continuous spectrum. To obtain the true, undisturbed radiation of an atom, we must isolate it from its neighbours.

In a gas or vapour, the atoms are known to be far enough apart to exercise their functions unhindered. Accordingly, they yield their characteristic line spectra. Our model of the atom gives a very satisfactory explanation of the differences between flame, arc and spark spectra which we have already pointed out. In the bunsen flame, the exciting agency is not vigorous enough to remove the electron to a very distant orbit, and we get, therefore, only the few lines corresponding to passages between the innermost orbits. The electric arc is a very much stronger stimulant, and is able to remove the electron to greater distances from the nucleus. The arc spectrum is consequently much richer in lines than the spectrum of the flame. It seems natural to attribute the production of the "enhanced" lines to a similar effect, arising from a still stronger stimulus in the electric spark: there is now no doubt, however, that it has a more complex cause than this. We have already said that the spectra of elements containing more than one electron are due to the movements

of the outermost electron in each atom. The agency responsible for the luminosity removes an electron from its orbit, but, when embodied in the flame or the arc, is not able, as a rule, to detach it permanently. The electron returns, and radiates its surplus energy as a wave of light. But now, suppose the stimulus is so great that the outermost electron is removed quite beyond the sphere of influence of the rest of the atom, and does not return at all. We are then left with what is, for all practical purposes, a new atom, in which the depleted army of electrons is no longer able to balance the nuclear charge ; we have, in fact, a positively charged atom. The now outermost electron will assume the mantle of the fugitive, and proceed to move from orbit to orbit, thus radiating waves of light and giving spectrum lines. But these new lines are not identical with the old ones. The possible orbits in the charged atom are as different from those in the neutral atom as are the orbits of one element from those of another. The modified atoms, therefore, give a spectrum of their own. It is believed that this is just what takes place in the electric spark. An atom which has permanently lost an electron is said to be "ionised," and the enhanced lines constitute the spectrum of ionised atoms. In practice, as we have already observed, the spark spectrum is not totally dissimilar to the spectrum of the flame or the arc. There are lines which are common to all three modes of excitation. The reason is that ionisation is not a process which takes place simultaneously, at one fixed temperature, throughout the whole of a substance. Some atoms probably are ionised in a very hot flame ; others remain neutral even in the spark. It appears that, at any one temperature, a certain proportion of the atoms of a substance are ionised, and this proportion grows as the temperature rises : there is a more or less definite equilibrium ratio of ionised and unionised atoms at each temperature.[1] In the flame, the amount of ionisation is extremely small. In the arc, it is much greater, and arc spectra generally contain a considerable number of enhanced lines. The electric spark produces a still greater proportion of ionisation, so that the lines due to the ionised atom are stronger than they are in the arc and new enhanced lines are able to appear, while the lines of the neutral atom are weakened or die out altogether. One can imagine a still

[1] The ratio is probably modified by other physical conditions, but it seems to depend mainly on temperature.

more extreme temperature, at which ionisation would be complete, and the spectrum would contain enhanced lines alone. This has been attained on the Earth as yet only with a few elements, but there are certain classes of stars in which it appears to be almost the general rule.

Ionisation is, clearly, a process which can be repeated. An atom which has lost an electron may lose another, and still another, producing a fresh spectrum every time. Fowler has actually obtained evidence of a first, second, and third ionisation of the atom of silicon. In general, however, ionisation becomes more difficult at each step, for, as the successive electrons go, the atom is left with a stronger and stronger resultant positive charge, which holds the remaining electrons ever more firmly to their posts. As a consequence, laboratory research has so far been restricted, for the most part, to the spectra of the neutral and singly-ionised atoms. It is extremely important that the conditions of production of different spectra should be carefully studied, for it is only in this way that we can learn what is taking place in the stars and nebulæ. Within the last few years, the question of ionisation has become of very great importance in connection with the character of stellar atmospheres, and the amount of space we have devoted to it does not exaggerate the part it is likely to play in astrophysical theories in the future.

We have yet to consider the origin of band spectra in the light of our atomic model. Since a band is ultimately composed of lines, it is probably produced in essentially the same manner as a line spectrum. It is significant that, when the spectrum of a compound is obtained—under conditions which prohibit complete dissociation of the compound into its elements—it invariably shows bands. This has led to the conclusion, which is now generally accepted, that bands originate, not from single atoms, but from groups of atoms, or molecules. The molecules need not be composed of atoms of different elements, as they are in a compound : they may be molecules of a single element (see Plate VII. (5)). Considerable progress has recently been made in bringing to light the processes which underlie the production of bands.

Finally, a word must be said about absorption spectra. If emission of radiation is attributed to the balance of energy left over when an electron moves from an outer to an inner orbit, absorption must be regarded as the reception of energy from the source of light, resulting in the removal of an electron

from its normal position to an outer orbit. Thus, absorption is just a reversal of the process of emission. It is easy to see now why the absorption spectrum of a luminous vapour is identical with its emission spectrum. Since the possible orbits of the active electron are fixed, the outward passages, requiring the supply of energy, must be the exact reverse of the inward passages which result in the radiation of energy. Consequently, the amount of energy absorbed in the one process is exactly equal to that emitted in the other, and the spectrum lines produced by absorption are identical in position with those produced by emission.

The diameter of an atom is of the order of 10^{-8} inch, and an electron is far smaller than this. There are stars which we can see at distances of at least 2×10^{17} miles. There is something awe-inspiring in the thought that we see a star at this immense distance, because of movements of inconceivably small particles which took place tens of thousands of years ago. But Nature is full of wonder, and it is not only the poet who, from the meanest things, can draw " thoughts that do often lie too deep for tears."

CHAPTER II

CONTINUOUS AND LINE SPECTRA

It has been said that the Almighty must smile upon scientific research, or He would not have filled the Universe with so many clues to its character. Those who assent to this proposition will find support for their belief in the fact that the spectra of more than 99 per cent. of the stars are absorption line spectra. Now, an absorption line spectrum is precisely the kind of spectrum from which we can learn most. In the first place, it tells us something about the internal structure of its place of origin, for we know that, to produce absorption, we must have two sources of light—a hotter and a cooler—in certain relative positions and in certain physical conditions. In the second place, an absorption line spectrum has all the essential qualities of the two kinds of spectrum of which it is built up: it has the continuous background, due to the hotter source, and also the dark lines, arising from the interposed material. Whatever there is to be learnt from either of these phenomena is available for us in respect to the vast majority of the stars we can examine.

Inasmuch as identical continuous spectra can be yielded by all luminous solid or liquid bodies, we shall not expect them to be as useful to us as the more variable line spectra. They are, nevertheless, far from being without meaning. We have already observed that, when a solid or liquid body is heated, both the range of wave-lengths and the total quantity of radiation emitted increase with rising temperature. This at once suggests a means of measuring the temperature of a body by examining its spectrum: it seems that all we have to do is either to find how far the spectrum extends towards the violet (or region of shorter wave-length), or else to measure the total quantity of radiation in the spectrum, and immediately deduce the temperature in the light of previous experiments made to correlate the quantities involved. Let us examine the matter more closely.

In Fig. 4 curves are drawn, showing how the radiation of different wave-lengths, from a piece of platinum, changes

with changes of temperature. Each curve is labelled with the corresponding temperature, and its height at any point represents the quantity of energy in the radiation whose wave-length is marked along the horizontal line. The phenomena with which we are dealing will be at once apparent. Their use as a form of thermometer, however, now appears in a somewhat different light. To begin with, the limit of the

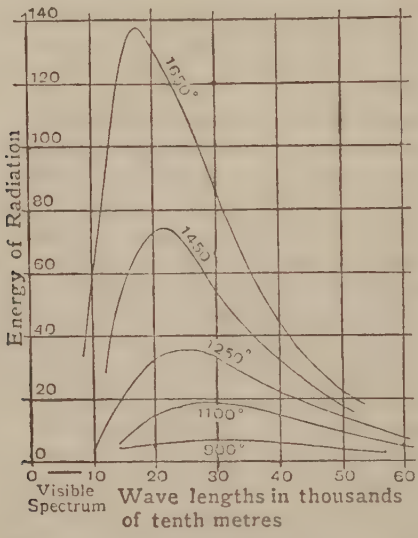


Fig. 4.—Curves showing Distribution of Energy in Spectrum of a hot Solid.

spectrum on the violet side [1] does not always appear to change very rapidly with temperature, so that the measurements would not reach a very high degree of precision. Moreover, in actual practice the spectrum fades out very gradually, so that its exact boundary cannot be determined. Finally, in its application to Astrophysics, there is one absolutely fatal objection to this criterion of temperature. The stars

[1] The region of shorter wave-length is generally referred to as the "violet" end, and the region of longer wave-length as the "red" end of the spectrum. Thus, a line is often said to be displaced to the "violet" when its wave-length is decreased, although, if the line is in the ultra-violet, such a displacement would, literally, be away from the visible violet region.

are intensely hot, and their spectra extend, therefore, far into the ultra-violet. But the Earth's atmosphere is an impassable barrier for all but the longest of the ultra-violet waves. Consequently, all stellar spectra appear to extend to the same point—the point at which they are cut off by our atmosphere ($\lambda 2950$). Our first idea of measuring the temperatures of stars, by the range of their spectra, must therefore be abandoned.

But there is still a second method, depending on the total quantity of radiation emitted. This is more practicable, and is, in fact, used in terrestrial experiments when exceptionally high temperatures have to be measured. But, for astrophysical purposes, there is again a very serious drawback. The radiation in the spectrum is the radiation we receive ; it is not the whole radiation of the star. Nor is it a complete index to the full radiation. Apart from the fact that a great deal of energy is absorbed by the Earth's atmosphere—a loss for which a suitable correction can be applied—the energy received will depend on the distance of the star from the Earth. The more distant a star, the less energy will the Earth receive from it for a constant total radiation. Moreover, to make the measurements reliable, the energy examined must be radiated from a standard area, for, obviously, a surface of two square feet will give more radiation than a similar surface of one square foot at the same temperature. In a stellar spectrum we have light from a whole hemisphere of the star, and, since the surface areas of stars differ from one another, we cannot use the total radiation for the purpose of a temperature classification unless we take this into account. We see, therefore, that this second method of measuring stellar temperatures also falls to the ground, for the number of stars for which we know both the distance and the surface area is almost negligible.

Of what use, than, it will be asked, is the continuous spectrum in Astrophysics ? If we look at Fig. 4 again, we shall see that there is another characteristic of the curves which we have overlooked. At any one temperature, the amount of radiation is not the same for all wave-lengths. The energy is distributed in the spectrum in such a way that more of it is embodied in waves of one particular length than in any other of the waves. Further, this wave-length of maximum energy—represented by the highest point on a curve—is not the same at all temperatures : it is smaller at

higher temperatures than at lower ones—a condition represented by the movement of the peak of the curve to the left as the temperature rises. This effect has been examined carefully, and it has been found that there is a very definite relation between the wave-length of maximum energy in a continuous spectrum and the absolute temperature [1] of the source of radiation. If λ_m denotes this particular wave-length in A.U., and T is the absolute temperature of the source, then the product $\lambda_m T$ is found to be constant for all values of the temperature, and equal to 29.40×10^6. The use of this relation in measuring temperatures is obvious. It is necessary only to observe at what point of the spectrum the maximum amount of energy appears and to divide the wave-length corresponding to that point into the number 29.40×10^6, in order to obtain at once the absolute temperature of the source. This method, with certain refinements, is actually applied to the measurement of the temperatures of stars.

There is one important precaution to be taken, however, in estimating temperatures by means of this principle. The radiation from any piece of matter depends, not only on the temperature, but also on the nature of the surface from which the radiation issues. A bright, polished surface, for example, radiates much less, at the same temperature, than a dull black surface. There is a certain ideal surface—to which lampblack approximates very closely—which is regarded as a "perfect radiator"; *i.e.* it radiates the theoretical maximum of possible radiation corresponding to its temperature. It is only to such a surface that we can rigidly apply the relation, $\lambda_m T = 29.40 \times 10^6$. For any other radiating area, the value of λ_m, for each value of T, will be greater than it is for the perfect radiator, and the calculated value of T will therefore be too small. Unless the stars are perfect radiators, therefore, the temperatures measured by this method will be less than the true values, and all we can say of our results is that they represent the minimum possible values of stellar temperatures. It is believed that the effective surfaces of stars do not give much less than full radiation, so that our measurements are probably not very much too low.

[1] The *absolute* temperature is the temperature on a scale whose zero represents a condition of complete absence of heat, and whose degree is equal to the Centigrade degree. The absolute temperature of a body is obtained by adding 273 degrees to its Centigrade temperature.

If we recall the origin of the continuous spectrum, we shall see that temperature is just the type of property it might be expected to indicate. The continuous spectrum is the product of the whole body of a substance. Starting originally from discrete atoms, the radiation loses its identity by continual atomic collisions and interactions, and issues as a beam bearing the hall-mark of an aggregate of atoms rather than that of the individual atoms themselves. Now temperature also has this quality : it is determined by the mean velocity of the atoms or molecules of a body which, individually, might have speeds varying over a wide range. Consequently, a correlation between temperature and the continuous spectrum is quite in keeping with our physical ideas. We might, indeed, go further, and look for relations between the continuous spectrum and other mass—or average—qualities of the luminous source. There is mechanical pressure, for instance, or velocity, or magnetic condition : these are all mass-effects, and have, in fact, been found to influence the emission of radiation. Might not the continuous spectrum reveal the state of a body with respect to qualities such as these ? There is every probability that it might, theoretically ; practically, the measurement of the qualities from a continuous spectrum is impossible, for the following reason. Each of these states is represented in the spectrum by a slight displacement of the radiation to the left or right, or else by a separation of one wave-length into two or more closely adjacent ones. In order to detect it from a continuous spectrum, we should therefore have to measure an extremely small displacement of the whole spectrum with respect to a normal one, or else the slight overlapping of colours due to the splitting of the individual wave-lengths into components. In a continuous spectrum, where the colours merge insensibly into one another, and the spectrum dies out with perfect graduation on either side, there is no practicable means of doing this. It is much more conveniently done with the fine, narrow slit-images of a line spectrum, in a manner which we shall consider in the next chapter. In practice, therefore, the only use that has so far been found for the continuous spectrum is the measurement of temperature.

In coming to the line spectrum, we approach the heart of spectroscopy. We have called it a projection of the atom

itself, and so, indeed, it is, inasmuch as no detail of any theory of the atom can be accepted unless it can show its influence on the line spectrum. In the apparently chaotic mass of lines that issue from the atoms of a single element, the strictest regularity is found to exist—a regularity which is but the outward and visible sign of the structure of the atoms themselves. When physicists first applied the spectroscope to the light from glowing gases, they entered a road which apparently leads to the end of the world. The first milestone was the identification of the chemical elements; the latest is the knowledge of the structure of matter; and still the end is lost in the distance. In the remainder of this section we will retrace, very rapidly, the ground that has been covered, for, remarkable as it may seem, there is scarcely a discovery of the laboratory that has not found an application to, or been directly inspired by, the spectra of the heavenly bodies.

Spectrum analysis is the best known, and was once the only, application of spectroscopy. Its possibility arises from the fact that the line or band spectrum of a luminous vapour—although it may vary considerably with the conditions of excitation of luminosity—is always characteristic of the chemical composition of the vapour. If a substance contains several elements, the lines of all of them appear in the spectrum under appropriate conditions. Spectrum analysis, thus explained, seems the easiest thing in the world. All that one has to do is to vaporise and make luminous the substance, look at the resulting spectrum, and say what elements are present. As usual, however, unexpected difficulties arise when the operation is attempted. In the circumstances of the experiment, some elements may evade their responsibilities and give no spectrum at all—as, for instance, does chlorine when a metallic chloride is placed in an electric arc, or the rare gases of the atmosphere when an electric discharge passes through air at a low pressure. Others again—especially if they are present in small quantity—may content themselves with giving only one or two lines, and those lines may be so close to lines of other elements as to be hardly distinguishable from them. Difficulties such as these make it necessary to study the conditions under which different classes of spectra are excited, and to be able to identify the lines of any spectrum by the most exact measurements of wave-length that are possible. The second requirement

perhaps still affords the chief occupation of the practical spectroscopist, for all spectrum lines are identified ultimately by the wave-lengths of the light in them.

The actual course of a spectrum analysis, is, in principle, somewhat as follows. The spectrum of the unknown substance is obtained in different ways, and the wave-lengths of the lines produced are measured. They are then compared with tables giving the wave-lengths of lines corresponding to the different chemical elements, and the identification is made. Direct measurement of wave-lengths, however, is a very laborious matter, and is generally not undertaken in routine work. Standard wave-lengths have been measured with great accuracy—especially the lines of the iron arc spectrum, which are extremely numerous—and the wave-lengths of unknown lines are calculated from the positions of the lines with respect to the standards. To facilitate the measurements, and to make them as accurate as possible, the iron arc and the unknown source are exposed to the spectroscope successively. Each is exposed to a different portion of the slit, the two portions having a short length in common. The resulting spectrum contains the lines of iron and the unknown lines in separate rows which just overlap one another, as in Plate VII. (1). The wave-lengths of the unknown lines are then determined from their positions relative to the standard iron lines along the overlapping portion of the two spectra.

In dealing with the spectra of heavenly bodies, the difficulties of analysis are in general greater than those arising in the laboratory. The conditions of luminosity cover a range much wider than our experimental limitations will allow us to imitate at present, and the spectra of familiar substances may, and often do, appear in unfamiliar form in the analysed light of the stars and nebulæ. The solar spectrum, for example, contains a large number of lines which have not yet been identified. This does not mean that there are many unknown elements in the Sun. On the contrary, the lines are believed to be due to known substances, under unaccustomed excitation. The number of such lines which have been so assigned since the solar spectrum was first studied lends strong support to this view. Another difficulty which stellar spectra present to the analyst arises from the fact that some of the agencies causing displacement of lines in the spectrum may be more powerful in the stars than they are on the Earth. When an

identification rests upon a single line, appearing in the midst of a crowd of other lines, this may cause great uncertainty. Coincidence of stellar lines with lines produced in the laboratory does not necessarily mean identity of chemical origin. There is a considerable list of elements whose presence in stellar sources is still in doubt.

It will be realised that spectrum analysis is not, in practice, the simple thing it appears to be. Nevertheless, by careful study of the influences to which lines are susceptible, and ever increasing experience of the behaviour of substances on the Earth, a high degree of reliability has been attained. It remains for us to consider the disturbing effects which, though detrimental to the progress of chemical analysis, are full of meaning when they are rightly understood.

CHAPTER III

THE DISPLACEMENT AND SPLITTING OF SPECTRUM LINES

The property of the atom which, above all others, impressed the physicists of the nineteenth century was its extreme stability. "If," wrote Clerk Maxwell in 1870, "we wish to obtain standards of length, time, and mass which shall be absolutely permanent, we must seek them not in the dimensions, or the motion, or the mass of our planet, but in the wave-length, the period of vibration, and the absolute mass of these imperishable and unalterable and perfectly similar molecules." And again, a few years later: "Though, in the course of ages, catastrophes have occurred and may yet occur in the heavens, though ancient systems may be dissolved and new systems evolved out of their ruins, the molecules out of which these systems are built—the foundation stones of the material universe—remain unbroken and unworn." It was the firm conviction of the nineteenth century, engendered chiefly by what we now know to be the relatively gross changes involved in chemical reactions, that the forces of Nature were powerless, not merely to destroy, but even to modify the atom in the slightest degree.

This comfortable, but barren, belief is no longer tenable. By one of those revolutions so common, not only in Science but in every department of human thought, the difficulty is now, not to disturb the atom, but to find the conditions under which it can remain undisturbed. When we turn to the most intimate of all atomic phenomena—the spectrum—we find that scarcely two spectra of the same substance are exactly alike. A change of almost any kind in the physical conditions under which light is produced causes a corresponding change in the wave-lengths of the radiation emitted and, consequently, a shifting of the lines in the spectrum. It is no longer sufficient to say, for example, that the wave-length in air of the red line of cadmium is 6438.4696 A.U. In order to be precise, we must say at least that, for the

line to have this wave-length, it must be produced by an atom at rest with respect to the observer, acting in a region free from mechanical pressure and from electric or magnetic fields, where the acceleration due to gravity is 981 centimetres per second per second; and must be measured in dry air at a pressure of 760 mms. of mercury and a temperature of 15°C. The production of these conditions, or a sufficiently close approximation to them, requires no small amount of care on the part of the experimenter.

We shall turn our attention in this chapter to the various causes of displacement of spectrum lines, in so far as they have a bearing on astrophysical investigations. The influences which we shall consider are: relative motion between the source of light and the observer, pressure in the source of radiation, and magnetic and gravitational fields. The effect of an electric field on the spectrum, though of considerable theoretical importance, has not yet entered into Astrophysics.

When a source of radiation and an observer are either approaching or receding from one another, theory requires that the wave-length of the radiation, as measured by the observer, shall be slightly different from what it is when the relative motion does not exist. This is known as the Doppler principle. It can be, and has been, verified experimentally for the aerial waves which constitute sound. It is a familiar experience that an engine whistle appears to change its note abruptly when the engine passes a listener standing on the station platform. The relative motion changes from approach to recession, and the sound waves received accordingly change in pitch. For light waves, however, a proof quite so direct as this is not yet within the bounds of possibility. Light moves so exceedingly quickly compared with the relative speeds experimentally attainable, that the effect would be immeasurably small. Further, when we are dealing with etherial waves, the theoretical aspect of the matter is by no means so simple as it is with sound. The following elementary treatment, for the case when the source is moving towards the observer, gives an approximately correct result.

Suppose the source, S, and the observer, O (Fig. 5), are relatively at rest at a distance apart of V, numerically equal to the velocity of light, *i.e.*, to the distance travelled by light in one second. A train of waves issuing from S will reach O at the end of one second, and if λ is the wave-length of the

light, and n its frequency, then there will be n waves in the length SO, and we shall have

$$V = n\lambda.$$

Now suppose that, while the source is emitting waves, it is moving towards O with velocity v. Then, if it is at S at the beginning of the second, it will be at S' at the end, where $SS' = v$. The n waves emitted during the second will now be contained in a length $S'O = V - v$, for the first wave was emitted from S, and therefore would just reach O in a second, as before. Consequently, the length of each wave must now be λ', where

$$V - v = n\lambda'.$$

From the two equations, we have at once $\frac{\lambda'}{\lambda} = \frac{V - v}{V}$; or, if the change of wave-length, $\lambda - \lambda'$, be denoted by $d\lambda$, we have

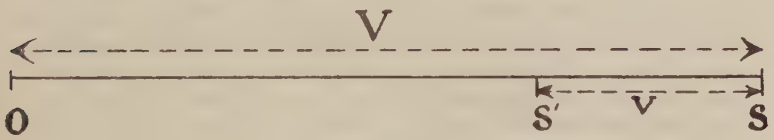


Fig. 5.

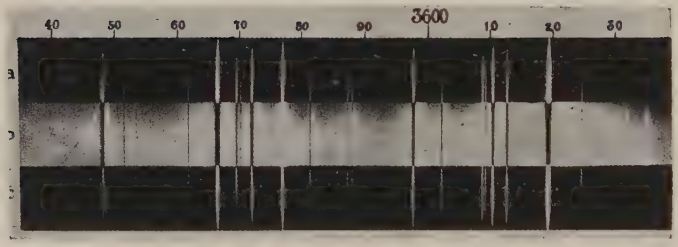
$d\lambda = \lambda \cdot \frac{v}{V}$. Now since the position of a line in the spectrum depends on the wave-length of the light in it, this change is equivalent to a shifting of the line towards the violet, compared with its normal position, by an amount depending on $d\lambda$. Knowing the normal wave-length of the light, λ, and the velocity of light, V, we can therefore, by measuring the shift, $d\lambda$, calculate v, the velocity of approach of the source. A similar treatment can be given to the case in which the source is receding from the observer, or the observer is approaching or receding from a stationary source. In every case, relative approach gives a displacement to the violet, and relative recession gives the opposite effect.

This argument, which is apparently irrefutable, is, nevertheless, a false one ; that is to say, the physical ideas it gives cannot represent reality. What we actually observe, in the light from a large number of stellar sources, is a displacement of the spectrum lines by amounts proportional to their wave-lengths. Assuming this to be due to relative motion in the line of sight, the " line of sight " velocities of the stars are

calculated, and figures are obtained which are consistent with what we should expect from more direct measures of velocity components. Other applications of the same principle also are found to agree with independent methods of investigation; *e.g.*, the known velocities, with respect to the Earth, of the Moon, Venus, and Mars agree with the velocities calculated from the spectrum line displacements of sunlight reflected from these bodies. There is very strong evidence, therefore, that a Doppler effect in light does exist, of the order calculated. But when we consider the physical cause of the effect, we are on much more debatable ground. The explanation we have given throws the responsibility for the displacement on the actual light waves in the Ether. That this is not justifiable can be seen at once, when we remember that the displacement is found only when there is *relative* motion between the source and the observer. If, in our example, the observer, like the source, were to move to the left, with velocity v, the displacement would disappear, yet all we have said about the length of the waves would be unaffected. The Doppler effect arises only from the relations between the source and the observer; it has nothing to do with the source alone, or with the observer alone, or with the Ether between them. If we conceive of the Ether as an objective reality, and "length" as an inalienable property of the waves it transmits, then we must absolve it from any responsibility for the Doppler effect. The whole matter receives a satisfactory explanation in the light of the theory of relativity, but it would take us too far afield to introduce that subject here. It will suffice for our purpose to say that theory and a strong body of experimental evidence assure us that we can employ the Doppler principle with confidence in astrophysical researches.

We accept, then, the principle that displacements of spectrum lines, by amounts proportional to their wave-lengths, may be attributed to a movement of the source or the observer along the line joining them. If there is relative motion along some other direction, then the velocity calculated according to the Doppler principle will be only the component, in the line of sight, of the full velocity: thus, in Fig. 6., if ST represents the direction and magnitude of the velocity of S relative to O, the Doppler displacement will be proportional, not to ST, but to ST'. If the source is moving at right angles to the line of sight, there will be no Doppler displacement at all. It should

PLATE VIII.



Nickel Spark Spectrum at Different Pressures.

(*a*) and (*c*) One atmosphere.
(*b*) Eleven atmospheres.

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be noticed that the measured displacements do not tell us whether it is the source or the observer that is moving : all they reveal is the *relative* motion. Indeed, it is very doubtful whether there is any real difference between the two cases. No optical experiment has yet been able to distinguish between them, and the conviction is gaining ground that the distinction itself is non-existent.

The displacement of spectrum lines by mechanical pressure in the source of radiation is a phenomenon of quite another kind. Here we are dealing with an effect arising from an actual modification of the atom : it has nothing whatever to do with the observer. We have seen that the origin of a continuous spectrum is to be assigned to the mutual disturbing influences of the radiating atoms which, in a solid or liquid, are closely packed together. In a gas the atoms are free and radiate their individual lines. Now the effect of increase

Fig. 6.

of pressure, whether by actual compression of the luminous gas or by the introduction of other gases, is to decrease the average distance between the atoms. Consequently, the first visible result is that the spectrum lines are broadened—a step towards a continuous spectrum. But, in addition to this, the centres of the lines are displaced—generally towards the red end of the spectrum, the region of longer wave-length. Plate VIII. shows the change of character of the lines of nickel when the pressure is increased. The displacements are, in general, too small to be seen clearly in the figure ; the line at $\lambda 3548$, however, is distinctly shifted to the right. It must be admitted that the cause of the displacement is not absolutely known. A large number of atoms in a gas are radiating the same line at any instant, and the broadening suggests that they are influenced by their surroundings to different extents, so that the actual wave-lengths radiated vary over the range corresponding to the enlarged width of

the line. If this is so, then the displacement of the strongest portion of the line towards the red means that the majority of the atoms are so influenced that they radiate less energy than they would do in the free state. It is to be expected that the equilibrium states of an atom will be altered to some extent by the influence of neighbouring atoms, so that the possible orbits of the spectrum-giving electron will be changed. Some amount of success has, in fact, attended efforts to explain part of the effect as a result of electric fields surrounding the neighbouring atoms and arising from the electric charges in them. It is not altogether clear, however, why the displacements are usually towards the red. It is possible that light will be thrown on the matter from the fact that the pressure effect is not the same for all lines, even in the same spectrum. Some lines are modified much more than others, though the change is always of the same character. The difference of behaviour is well represented in Plate VIII. It may be that a study of the orbits associated with the lines, in relation to the magnitude of the pressure modification, will give a clue to what is taking place. In this connection, it may be noted that the lines forming bands show practically no pressure effect: it seems as though molecules are much less influenced by pressure than are atoms.

The explanation of the effect, however, is of secondary importance for Astrophysics: it can be left to the spectroscopist to pursue. From the astrophysical point of view, experimental study is the first consideration, and, unlike the Doppler displacements, the pressure modification offers no difficulty in this respect. It appears that, for the same line, the displacement is roughly proportional to the pressure in the source. We have, therefore, a means of estimating the pressures in those parts of the stars which give rise to the lines in the spectra. The displacements of the most sensitive lines must be measured, and the pressure deduced from terrestrial observations. If there is a Doppler displacement as well, the two effects will, of course, be superposed, and the actual displacement will be the resultant of the separate ones. There is no difficulty in distinguishing them, however, for the Doppler displacement affects all the lines, by amounts proportional to the wave-lengths, whereas the pressure effect is selective and leaves some lines unmoved.

The effect of a magnetic field upon spectrum lines is one

aspect of the fundamental connection between magnetism and light—a connection which, perhaps more than any other, has helped to establish the conviction of the essential unity of physical phenomena. Faraday dreamed of it ; indeed, he showed that light itself was modified when passing through a magnetic field, though he was not able, with his restricted apparatus, to detect the influence of such a field on the actual radiating atoms. Clerk Maxwell, by his electro-magnetic theory of light, made the effect more than a vague possibility, and inspired other attempts to succeed where Faraday had failed. But it was not until 1895 that the effect was clearly demonstrated. Zeeman, guided by theoretical predictions of Lorentz, placed a source of light between the poles of a very powerful magnet, and observed the spectrum lines. At first he found merely a broadening of the lines—which, though not the full confirmation of Lorentz's theory, was evidence, for the first time, that a magnetic field could influence a radiating body. Continuing his experiments, Zeeman soon found that the broadening resolved itself into a splitting up of the lines into components. In the normal case, a line was split into two, symmetrically placed on either side of the position previously occupied by the " unmagnetised " line, provided that the source was examined along the direction of the magnetic field. If it was observed in a perpendicular direction, the original line was seen, as well as the adjacent components. Now this was exactly in agreement with the theory of Lorentz, except that the distances between the components of the dissected line were smaller than the predicted ones. Lorentz had assumed that radiation was the result of a vibration of the whole atom. The observations of Zeeman could be accounted for if the vibrations were attributed to a small part of the atom—a part whose relative mass came out to be just that which we now assign to an electron. At that time the electron had not been isolated, so that Zeeman's observations, in the light of Lorentz's theory, constituted a remarkable prediction of the existence of sub-atomic units of matter. There is one other deduction from the theory which Zeeman was able to verify. An ordinary beam of light consists of waves whose " vibrations " take place in all planes through the direction of propagation. It is possible to treat the light so that the vibrations are confined to a single plane. Light so treated is said to be " polarised " in the plane perpendicular to that containing its vibrations.

Thus, light consisting only of vertical vibrations is polarised in a horizontal plane. The eye can detect no difference between polarised and unpolarised light, but there are instruments which can show the distinction at once. Now Lorentz predicted that the two outer components of the lines, seen in the direction perpendicular to that of the field, should be polarised in a certain plane, and that the original line, between them, should also be polarised, but in a plane at right angles to that of the outer components. The lines seen in the direction of the field should be polarised in a rather more complex manner. Zeeman substantiated these prophecies in every detail.

Plate IX. shows the Zeeman effect for the lines of chromium. It should be noted that there are many lines which show an "abnormal" Zeeman effect: they are split up into more components than the theory of Lorentz predicts. The explanation of these exceptions, as well as of the normal effect, is being given by a modified theory, called for by the later conceptions of the structure of the atom which we have already outlined. It will be perceived that the Zeeman effect has many characteristics by which it can be recognised. If the magnetic field is strong enough to show the phenomena clearly, there is no danger of the observations being attributed to any other cause.

It is not quite certain whether or not a gravitational field has any influence on the position of a line in the spectrum. The possibility that there might be such an influence arises from Einstein's relativity theory of gravitation. It would, of course, be going outside the scope of this book to give a full account of the reasoning by which the effect is to be expected. In general terms, it is somewhat as follows. According to Einstein, space and time are not independent things, but are to be regarded as two aspects of a "space-time continuum," which is a medium for expressing the relations between events. The phenomena of gravitation, which have hitherto been attributed to a mysterious "force" arising from the inherent properties of matter, can be described by supposing that the space-time continuum is modified, in a manner defined mathematically, in the neighbourhood of heavy bodies. What we usually call the "strength" (or more strictly the "potential"), of a gravitational field becomes the amount of modification of the

PLATE IX.

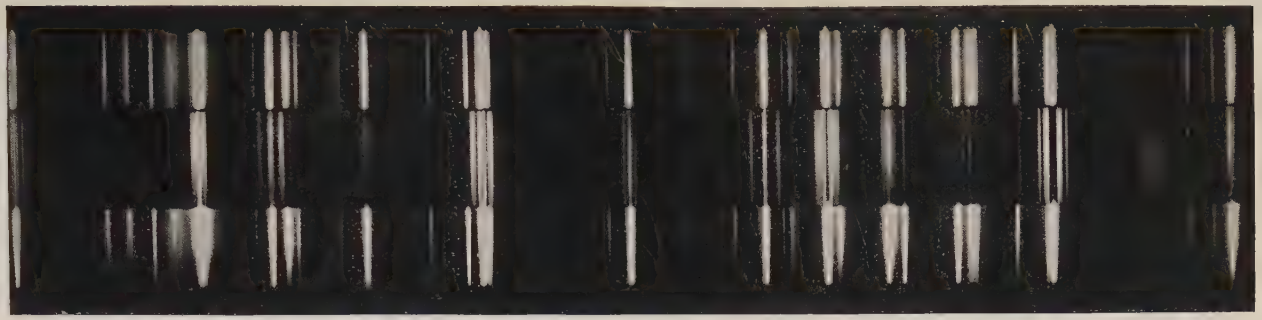

Photo: Babcock.

Spark Spectra of Chromium, showing Zeeman Effect.

(*a*) Without Magnetic Field. (*b*) With Magnetic Field.

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continuum compared with its state in a region free from matter. Now the modification of the space-time continuum means the modification of its "dimensions" or "sections," which we call space and time. Consequently, the "frequency" and "wave-length" of a wave, which involve the measurement of time and space, will vary with the gravitational field in which the wave originates, and the position of, say, a sodium line in the solar spectrum will be slightly displaced towards the red, on this account, from its position in a terrestrial spectrum. The effect, if it exists, is extremely small, and the difficulties of detection are very great. While some observers claim to have established the reality of the displacement, others, with at least equal care and experience, find no evidence of it at all.[1] The problem is further complicated by the disagreement among mathematicians, as to whether the relativity theory really demands such a displacement. Some hold that it does not, while others, again, maintain that we have not sufficient data to come to a definite conclusion on the matter. In the present unsatisfactory state of the problem, therefore, the effect can yield no information of value to Astrophysics. We mention it because, if it should ultimately be established that the effect exists, and our measuring instruments improve so that we can measure it with a fair amount of accuracy, we might have a spectroscopic means of determining the masses of stars. If this hope is ever realised, it will bring about a great advance of Astrophysics, for, as far as we can see, the only means of measuring the masses of the heavenly bodies is through their gravitational influence, and unless a star is double or multiple, it is too far from its neighbours to be affected appreciably by them.

We have considered the effect on spectrum lines of relative motion between source and observer, of pressure in the source of radiation, of a magnetic field at the source, and of the gravitational state of the region from which the radiation issues. With regard to the first three of these effects, there is no difficulty in distinguishing one from another. The velocity, or Doppler, displacement is proportional to the wave-length of the line. The pressure effect bears no simple relation to wave-length; the most sensitive lines have to be determined empirically. The Zeeman effect involves a

[1] Since this was written, the balance of evidence has definitely turned in favour of the existence of the displacement.

splitting of the lines and certain polarisation phenomena. An electric field also, which we have not had occasion to consider, produces characteristic modifications of the spectrum lines. But the hypothetical gravitational displacement has no mark to distinguish it from the Doppler effect : it also varies as the wave-lengths of the lines displaced. And herefrom arises an uncertainty as to exactly how far we can place reliance on measurements of stellar velocities by the Doppler principle. After allowing for the influence of pressure, we attribute the whole of the observed displacement to relative velocity. If a part of the displacement actually arises from the gravitational field of the star, there will be a systematic error in our deductions. Velocities of approach will be estimated too small, and velocities of recession too large. Moreover, there may be other influences at work, which we have not yet discovered. Velocity measurements must therefore always be somewhat dubious. But it is very unlikely that there is any appreciable error in the measurements. With the Sun, at least, the results have been checked by an independent method. The period of rotation of the Sun on its axis can be determined by the Doppler principle, from the velocities of the receding and approaching limbs, and also from the times between successive passages of sunspots across the centre of the Sun's disc. The two methods give results in very good agreement, so that here, at any rate, the Doppler principle is to be relied upon. It would be rash to generalise from a single example, but the general order of magnitude of the estimated line-of-sight velocities accords so well with what we should expect from the changes of apparent position of stars in the sky, that there is every justification for placing considerable confidence in them. Nevertheless, if the results are ever found to be at variance with the indications of independent means of investigation, the possibility of misinterpretation of the displacements will have to be taken into account.

CHAPTER IV

THE ANALYSIS OF SPECTRA

So far we have considered the spectrum only as a whole or with regard to particular lines. We turn now to the relations which exist between the positions of the lines in the spectrum of a single element. This is the department of spectroscopy that has led to most of the knowledge we possess about the structure of the atom. The spectrum, as a whole, identifies an element; the analysis of the spectrum describes it. It is the regularity found in the spectrum that indicates and makes plain the internal regularity of the atom, and it is just here that Astrophysics does the greatest service to the study of matter. The laboratories of the stars deal with atoms in a way as yet impossible to us on the Earth, and through the spectrum we can see what the atom is capable of doing under conditions which we are not able to reproduce.

The idea that the lines of a spectrum are not unrelated to one another can hardly fail to occur to any one who looks at the spectrum of the hydrogen atom (Plate XI. (1)). Here we have evidently a regular series of lines, getting closer together as we move towards the violet end of the spectrum, and, at the same time, getting fainter and fainter. This is the most obviously regular spectrum known—not a surprising thing, seeing that hydrogen is the lightest known element and has probably the simplest atom. Nevertheless, it was not until 1885 that the law of progression of the lines was discovered. In that year Balmer announced that the wave-lengths of the successive lines were obtained with great accuracy by giving successive integral values (3, 4, 5, etc.) to m in the formula

$$\text{Wave-length } (\lambda) = 3646.13 \frac{m^2}{m^2 - 4}$$

Assuming this formula to have a theoretical basis, two results at once follow. The strong line at $\lambda 6563$ must actually be the first line of the series, for it is obtained by putting $m = 3$. Smaller values of m give either infinite or negative values of the wave-length, which cannot represent real lines.

Secondly, there can be no line in the series whose wave-length is smaller than 3646.13, for this is the value of λ corresponding to an infinite value of m, and smaller values of m give greater wave-lengths. We have, therefore, a series of lines occupying a limited length of the spectrum. The characteristics of the series are better expressed by a formula which gives wave-numbers instead of wave-lengths. The wave-number is obtained by dividing the wave-length into 10^8, so that Balmer's formula can be transformed at once into the expression

Wave-number (ν) =

$$27418.75 - \frac{109675}{m^2}, \text{ or } \frac{109675}{2^2} - \frac{109675}{m^2}$$

where m can have any integral value from 3 upwards. The wave-numbers are thus seen to be expressed by the difference of two quantities, one constant and the other varying from line to line. The constant quantity, 27418.75—which is the wave-number of the shortest possible line in the series, λ3646.13—is called the "limit" of the series, and the variable quantity, which is subtracted from it, is called the "term."

The Balmer series, as we have said, occupies a definite length in the spectrum, limited at both ends. There can be no lines of the series outside the limits. But that does not mean, of course, that the hydrogen spectrum cannot extend further on either side: the Balmer series need not represent the complete spectrum. As a matter of fact, there are lines of hydrogen both in the far ultra-violet and in the infra-red, and these lines are found to fall into series of their own, of exactly the same type as the Balmer series. The ultra-violet lines (the "Lyman" series), for example, are represented by the equation

$$\nu = 109675 - \frac{109675}{m^2}, \quad \text{or} \quad \frac{109675}{1^2} - \frac{109675}{m^2},$$

where m may have any integral value from 2 upwards. The infra-red lines (the "Ritz" series), similarly follow the formula

$$\nu = 12186.11 - \frac{109675}{m^2}, \quad \text{or} \quad \frac{109675}{3^2} - \frac{109675}{m^2},$$

where the lowest value of m is 4.

It appears, then, that there is no essential difference between the limit of a series and its terms. The limit of the Balmer series $\left(\frac{109675}{2^2}\right)$ is a term of the Lyman series: the

limit of the Ritz series is a term of both the Balmer and the Lyman series. It seems that we can generalise the hydrogen spectrum by associating with it a number of terms, $\frac{N}{1^2}, \frac{N}{2^2}, \frac{N}{3^2}, \ldots$ (where $N = 109675$), whose differences give the wave-numbers of the lines which we see. Apparently, the difference between any two terms gives the wave-number of a possible line in the spectrum. All the lines whose higher term is the same compose a series such as the one illustrated in Plate XI.

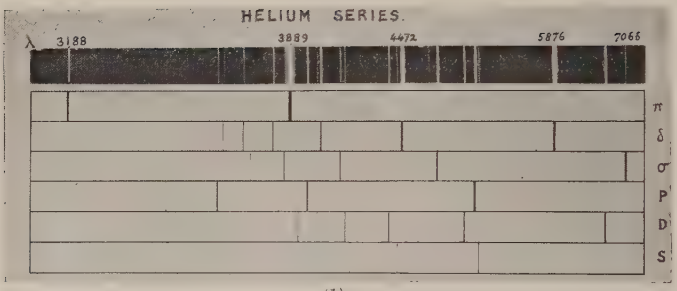
The reader will probably have no difficulty in interpreting these facts in the light of the model of the atom we have already presented. We saw that a line of the hydrogen spectrum was produced when the revolving electron moved from an outer to an inner orbit : thus, to quote our own words, " if the electron is removed from orbit No. 1 to orbit No. 4 (Fig. 3), and returns to orbit No. 2, one line of the hydrogen spectrum is produced, whose frequency is proportional to the difference of atomic energy corresponding to the states 4 and 2." We now see that there is a line of the hydrogen spectrum whose frequency (measured by wave-number), is proportional to the difference between the quantities $\frac{109675}{2^2}$ and $\frac{109675}{4^2}$. The inference is obvious. If we assume that the term $\frac{109675}{2^2}$ is proportional to the energy of the atom when the electron is in the orbit 2, and the term $\frac{109675}{4^2}$ is proportional to the corresponding amount of energy for the orbit 4, the facts and the theory are at one. With each orbit is associated a term, which represents the energy of the atom when the electron is in that orbit. A particular series is generated by passages of the electron to the same orbit. Thus the lines of the Balmer series correspond to passages from orbits 3, 4, 5, etc., to orbit 2, and so on.

The falling off of intensity, or weakening, of the lines as we proceed to the shorter wave-lengths also follows at once from the theory. The intensity of a line will obviously depend on the number of atoms which are sending it out together. The greater the number of atoms radiating a particular wave, the greater will be the amount of light in the corresponding line. Now, considering the Balmer series

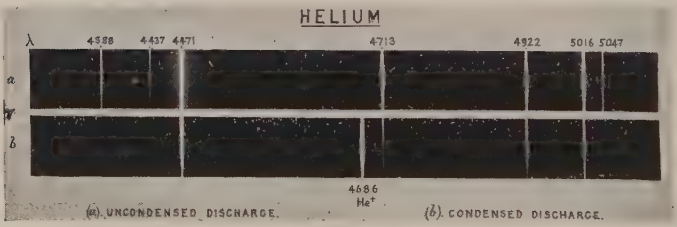
for example, every atom which has its electron displaced beyond orbit No. 2 can give rise to the first line, for the electron can return to orbit No. 2 step by step. The second line, however, can be given only by atoms whose electrons have been sent beyond orbit No. 3—for it is generated by direct returns from orbit No. 4 to orbit No. 2—and this must be only a fraction of the former number. Similarly, to radiate the third line, an atom must have its electron displaced beyond orbit No. 4, and so on. The chance of a line being radiated decreases as the higher members of the series are approached, and the series therefore shows a regular falling off of intensity. The consequence of this is that, instead of getting the infinite number of lines which are theoretically possible, we obtain only the first few members of the series, the others being too faint to be seen or to affect the photographic plate. It is not at present possible in the laboratory to photograph more than twenty members of the Balmer series, though, under the severer conditions existing in some of the stars, more than thirty can be detected. At the relatively great distances from the nucleus of the higher orbits, an electron would be in danger of being detached altogether from the atom by the influence of neighbouring atoms, so that we should not expect to obtain many more lines of the series than this.

The agreement between observation and theory in the matter of the hydrogen series is very striking, but it must be admitted that the theory was constructed in order to account for precisely these facts. The phenomena of the Balmer series were known long before Bohr adapted Rutherford's model of the atom to account for them. It is true that the Lyman series was predicted by Bohr before it was actually observed, but, with this exception, the theory accords with the facts, as we have stated them, because it was made to do so. The really striking thing about the Bohr theory is that it not only shows how the facts can be accounted for, but actually accounts for them numerically as well as in principle. We have assumed that the term $\frac{109675}{m^2}$ is proportional to the energy of the atom when the electron is in the mth orbit. Bohr not only assumed this, but, adopting certain principles that had been applied successfully to other electronic phenomena, he showed that the calculated expression for the atomic energy actually contained the number $N = 109675$

PLATE X.



(1)



(2)

Spectra of Helium.

To face page 47.

within the limits of experimental error.[1] It is this quantitative agreement, which has been extended also to other spectroscopic facts, that is the true passport of the Bohr theory. We shall not greatly concern ourselves here with numerical considerations; the physical ideas are what we require. But it is well to know that the mental images we form for our guidance have the sanction of mathematical analysis.

The simple spectrum of the hydrogen atom seems to be accounted for completely by the Bohr theory, supplemented by later refinements to account for the finer details. For other elements, the mathematical difficulties of calculating the energy of the atom in its various states have not been overcome. Mathematicians have not yet found a general method of predicting the motion of more than two bodies under their mutual influence, so that the spectra which, according to the theory, should be yielded by atoms containing a nucleus and more than one electron cannot be ascertained. The spectra themselves, for a large number of elements, have been analysed into series of the same character as those of the hydrogen spectrum, but the series often overlap to such an extent that their separation is by no means easy. Plate X. (1) shows the spectrum of helium, with the lines analysed into series. The letters *P*, *D*, *S* stand for "principal," "diffuse," "sharp"—the names given by Rydberg to three types of series which he found in a large number of spectra. Helium contains also another set of series, in which each line is double. The series of this group are represented in the figure by the Greek letters, π, δ, σ, which correspond to *P*, *D*, *S* of the singlets: the components of the doublets are too close together to be distinguished in the photograph. Some elements—for example, magnesium—contain series of triplets as well as of singlets. Others, such as sodium, appear to contain only doublet series. In most spectra there are a few lines which do not fall into the regular series, and there are a number of spectra in which no series at all have yet been detected. It is becoming evident that series relationships are not the only

[1] Bohr's expression for N is $\frac{2\pi^2 e^2 E^2}{ch^3} \cdot \frac{mM}{m+M}$, where e and m are respectively the charge (in electrostatic units) and mass of the electron, E and M are the corresponding quantities for the nucleus, c is the velocity of light, and h is "Planck's constant."

forms of regularity among spectrum lines. The full significance of this, however, is a matter for the future. The disentangling of the complexities of spectra is one of the most fascinating pursuits of spectroscopy, and the results can hardly fail to have a considerable bearing on the progress of Astrophysics.

We have said that, as a rule, spectrum series are of the same character as the series of hydrogen. By that we mean that they converge continuously to a limit on the side of shorter wave-length, and fall off in intensity as they do so. In details, the spectra of the heavier elements are much more difficult to understand than the comparatively simple hydrogen spectrum. No exact formula has been found to represent the general spectrum series, though close approximations are sometimes given by modifications of the Balmer equation. It is very interesting that the formulæ which are most successful in representing the lines involve the "series constant," N, which was first found for hydrogen. The Bohr expression for N (see footnote on p. 47) offers a satisfactory explanation of this. It would appear at first that N should vary considerably from one element to another, for it involves E and M, the charge and mass of the nucleus, and it is to the variation of these quantities that the multiplicity of chemical elements is attributed. But if we suppose E to be, not the nuclear charge alone, but the resultant charge of the whole atom with the exception of the revolving electron, then it becomes the same for all neutral atoms, and is numerically equal, in fact, to e, the charge of an electron. This is justified physically by supposing the revolving electron to be at a considerable distance from the rest of the atom, so that the latter can be regarded as a single body, acting at its centre with its resultant charge. The expression for N will then not differ, on account of the factor E, for different elements. But the mass of the nucleus, M, still seems to present a difficulty. As a matter of fact, the part it plays in the numerical value of N is very small—as may readily be seen by giving it various values, from 1 to infinity: the result is practically the same in all cases. The very gradual increase in N as M increases is too small to have any sensible influence on the imperfect formulæ which we have to employ for the spectrum series of the heavier elements. The universal character of the constant N is thus readily accounted for.

The Bohr theory, it would seem, is limited, so far as complete success is concerned, to the atom of hydrogen. It is baffled when faced by other atoms, because of the mathematical difficulties of the problem of three or more bodies. But there are circumstances in which its application can be extended. The neutral helium atom, for example, has two electrons, but if one of them is abstracted (*i.e.* if the atom is ionised), the remaining system, consisting of a nucleus and one electron, is amenable to investigation, and is, in fact, just like the atom of hydrogen except for the double charge and greater mass of the nucleus. The *doubly*-ionised lithium atom is again in the power of analysis ; and so on, throughout the table of elements. Owing to experimental difficulties, the singly-ionised helium atom is the only one of these systems that has so far been made to produce a spectrum, and it is one of the greatest triumphs of the Bohr theory that it accounts perfectly for the positions of the lines. There is an important difference between the spectrum formula for the ionised atom of helium and that for the neutral hydrogen atom : the value of N is vastly greater in the former. For, since the helium nucleus has twice the charge of the hydrogen nucleus, the value of E in the formula for ionised helium will be twice as great as in the formula for hydrogen, and since the formula contains E raised to the second power, the enhanced helium N is something like four times as great as the neutral hydrogen N. It is not exactly four times as great, because of the further modification arising from the different values of M for the two substances. We found that this modification was negligible in comparison with errors resulting from the imperfection of general series formulæ, but now that we are dealing again with an exact formula, it must be taken into account. The result is that, theoretically, the value of N for ionised helium should be a little more than four times as great as it is for neutral hydrogen, and the facts of experiment are in complete accordance with this requirement. If the doubly-ionised lithium spectrum could be obtained, it would, according to the theory, reveal a value of N a little more than nine (*i.e.* 3^2) times as great as the hydrogen N. Moreover, as Fowler was the first clearly to show, the enhanced spectra of heavier elements (arising from atoms which have lost *one* electron) contain series whose formulæ involve a constant practically four times as great as the constant

for the neutral atoms—just the result we should expect in the light of the Bohr theory.

All this seems very technical, and unlikely to be of any use to Astrophysics. It is, nevertheless, of vital importance, and contributes in large measure to modern ideas on stellar constitution and development. As an example of the interaction of astrophysical observation and spectroscopic theories, the story of the spectrum of ζ Puppis is well worth relating.[1] In 1896 the late Professor E. C. Pickering discovered a new series of lines in the spectrum of this star. In Plate XI. (2) the lines marked $H\beta$, $H\gamma$, etc., are the lines of the Balmer series of hydrogen, and the new lines are clearly shown in intermediate positions. There appeared, even at sight, to be a close relation between the two series, and the suggestion was substantially supported by an examination of the positions of the lines. It was found that the two series converged to the same limit, and were, in fact, closely represented by very similar formulæ. While the Balmer series, as we have seen, follows the formula

$$\nu = 109675 \left(\frac{1}{2^2} - \frac{1}{m^2}\right),$$

the new Pickering series appeared to be represented by

$$\nu = 109675 \left(\frac{1}{2^2} - \frac{1}{(m+0.5)^2}\right).$$

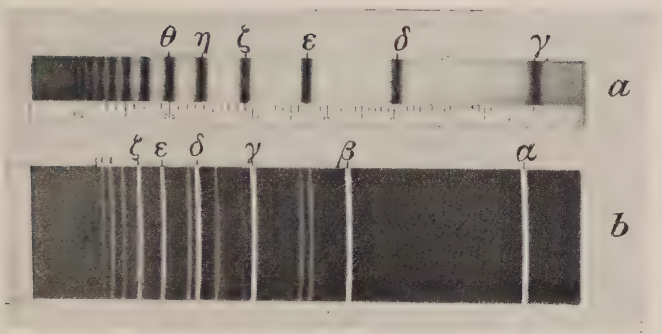
Now the relation between these two formulæ is precisely the same as that which was believed to exist between the sharp and diffuse series of the heavier elements, and Rydberg, who now took up the question, assumed that the Balmer series was actually the diffuse series, and the Pickering series the sharp series, of hydrogen. On this assumption, he calculated a hypothetical principal series, which would be represented by

$$\nu = 109675 \left(\frac{1}{1.5^2} - \frac{1}{m^2}\right).$$

The lines of this series would appear at λ4688, λ2734, etc. Now, λ2734 is too far in the ultra-violet to reach us through the Earth's atmosphere, but λ4688, if it actually existed, should appear as a bright blue line. As a matter of fact,

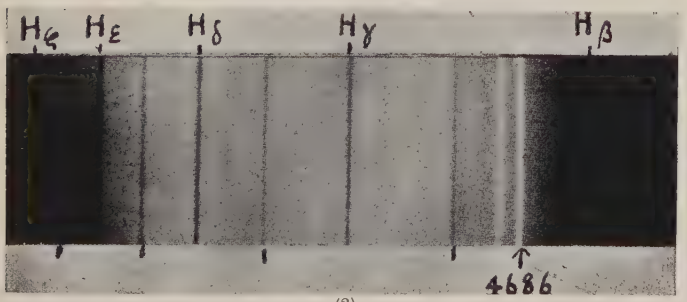
[1] For a complete account of this story, see Professor Fowler's address to the Royal Institution on "The Spectra of Hydrogen and Helium," delivered on 31st March, 1916.

PLATE XI.



(1)

The Spectrum of Hydrogen.
(*a*) In Sirius ; (*b*) in Vacuum Tube.



(2)

Spectrum of ζ Puppis.

To face page 50.

a line of about this wave-length (λ4686, to be more exact), was already known in the spectra of certain stars and nebulæ, and appeared as a bright line in the spectrum of ζ Puppis itself, as Plate XI. (2) shows. There seemed, therefore, to be a strong body of evidence that both the Pickering and the Rydberg series belonged to hydrogen, and though they had never been obtained in the laboratory, this belief was held for many years. The absence of the lines from laboratory spectra was attributed, plausibly enough, to our inability to reproduce the conditions prevailing in the celestial bodies.

For the next chapter of the story, we must turn to observations of the solar spectrum during times of total eclipse. On these occasions, the higher levels of the Sun's atmosphere can be studied without the intervention of the brilliant internal solar light. Fowler had already, in 1893, noticed a line at about λ4686, in the spectrum of the Sun's outer layers, but the significance of the discovery was not at that time realised. In 1898 Fowler again obtained the line, more strongly, from the same source, and at once conceived the possibility of its connection with Rydberg's predicted principal series line of hydrogen. At the same time a number of laboratory spectra were examined with the same instrument —among them, the spectra of some tubes of helium. One of these, by a remarkable coincidence, showed, for the first time from terrestrial sources, the line λ4686. Unfortunately, the tube got broken, so that it remained doubtful whether the line arose from helium, by some accidental peculiarity in the method of discharge, or whether it was due to hydrogen or some other impurity which happened to be present.

It was not until 1912 that the line appeared again in the laboratory. In the summer of that year—partly by accident and partly as the climax of several years' alertness—Fowler caught a very faint glimpse of it, once more from a helium tube. This time it was not allowed to escape. A systematic investigation revealed the fact that, if the electric discharge producing the luminosity were sufficiently disruptive, it could be produced at will, with almost any desired strength, together with other lines of the series calculated by Rydberg. Plate X. (2). shows the 4686 line, which is quite absent from the comparison spectrum obtained from the same tube under a more gentle stimulus. The measured wave-length (λ4685.98) was in very close agreement with the wave-length of the line

observed in celestial sources, and was believed to be sufficiently close to Rydberg's calculated line at λ4688 to justify its assignment to hydrogen. Hydrogen is a very common impurity in helium tubes, and it was known that it had somehow got into the tube with which Fowler's discoveries were made.

But now a quite unexpected difficulty arose from the fact that the observed Rydberg series contained more lines than Rydberg had calculated. There were lines between the calculated ones, just as the Pickering series appears with respect to the Balmer series (Plate XI. (2)). This was taken to mean that hydrogen possessed a second principal series, converging to the same limit as the first, and the lines were found to fit a formula quite in keeping with this supposition. Fowler obtained the Pickering series also from the same source, so that he felt justified in pronouncing the hydrogen spectrum to be more complex than it had at one time been regarded, and in assigning to it at least the Balmer series, the Pickering series, the Rydberg series, and the supposed second principal series which he had obtained.

Very likely the same view would have been held to this day had it not been for Bohr's theory of the atom, the beginnings of which appeared in 1913. To understand how this theory threw light on the matter, let us consider the formulæ which were found to represent the various series with which we are concerned. Writing, as usual, N for the number 109675, they are as follows:

$$\nu = N\left(\frac{1}{2^2} - \frac{1}{m^2}\right)$$ Balmer series: $\lambda = 6563,\ 4861,$ etc.

$$\nu = N\left(\frac{1}{2^2} - \frac{1}{(m+0.5)^2}\right)$$ Pickering series: $\lambda = 5414,\ 4544,$ etc.

$$\nu = N\left(\frac{1}{1.5^2} - \frac{1}{m^2}\right)$$ Rydberg series: $\lambda = 4688,\ 2734,$ etc.

$$\nu = N\left(\frac{1}{1.5^2} - \frac{1}{(m+0.5)^2}\right)$$ Supposed 2nd principal series: $\lambda = 3205,\ 2512,$ etc.

Now these formulæ can also be written, with the same numerical results, in the following way:—

$$\nu = 4N\left(\frac{1}{4^2} - \frac{1}{m^2}\right)$$ Balmer and Pickering series: $\lambda = 6563,\ 5414,\ 4861,\ 4544,$ etc.

$$\nu = 4N\left(\frac{1}{3^2} - \frac{1}{m^2}\right)$$ Rydberg and 2nd principal series: $\lambda = 4688,\ 3205,\ 2734,\ 2512,$ etc.

Regarded in this way, the first two series become one, as do

also the last two, and we have only two independent series to deal with. Now N, as we have seen, is, according to Bohr's theory, the characteristic of series proceeding from neutral atoms, while $4N$ is very close to the constant for singly-ionised atoms. We have, therefore, alternative explanations of the origin of our series: they are either four series from neutral atoms, or two series from ionised atoms. But now, if they are due to ionised atoms, those atoms cannot be hydrogen, for, since hydrogen possesses only one electron, it can have no means of producing a spectrum when it is ionised. The alternatives are, therefore, four series from neutral hydrogen or two series from ionised helium.

How can a decision be made between these two possibilities? As so often happens in science, everything depends on a minute difference between their respective requirements. The theoretical constant for ionised helium, as we have seen, is not exactly four times the value of N for hydrogen, because of the slight modification arising from the difference between the masses of the hydrogen and helium nuclei. While N for hydrogen is 109675, the constant for ionised helium should be 4×109720, and this value should be put for $4N$ in the formulæ we have just given, if the lines are generated by helium and not by hydrogen. The result of this is that there is a small difference between the wave-lengths calculated from the first set of formulæ and those calculated from the modified second set. Assuming the theory to be true, the origin of the lines will be determined by comparing the measured wave-lengths with those calculated in the two ways. Now we have already noticed a discrepancy between the predictions of Rydberg and Fowler's measurements of the observed lines. Rydberg's 4688, for example, was found actually to be 4686, and a similar discrepancy characterises the other lines which we are considering. But if we now substitute the ionised helium constant for $4N$ in the second set of formulæ, the calculated values are almost exactly equal to the observed ones, the differences being well within the limits of unavoidable experimental error. It has, therefore, been concluded that the Pickering, the Rydberg, and the supposed second principal series of hydrogen do not belong to hydrogen at all, but to ionised helium. To settle the question finally, Evans and others afterwards obtained the lines from helium which was shown to contain no trace of

hydrogen, so that there can now be no doubt whatever that helium is their true parent.

A word must be said about the Balmer series. It has long been known that it proceeds from hydrogen. It now appears that it can form the alternate members of a series proceeding from ionised helium. There can be no mistake, however, as to its origin in any particular case. The presence or absence of the Pickering lines shows at once whether or not it is due to ionised helium, and, moreover, the positions of the Balmer hydrogen lines are slightly different from those of the corresponding helium ones, because of the slight difference between the values of N for the two elements. Of the four series which for so long were attributed to hydrogen there is thus left but one legitimate claimant to the title. Abstract reasoning, confirmed later by actual observation, has decided the case.

And so it is throughout the whole of Astrophysics. Theory interprets observation and observation confirms theory. The physicist, studying in his laboratory, tumbles on the key to celestial evolution: the astronomer, roaming among the stars, finds electrons in new attitudes, and illumines the dark places of physics. In this brief account of the principles of spectroscopy, we have included nothing irrelevant to our subject: not a word has been said that has only an earthly significance. Yet, though perhaps from a distance, we have faced almost every aspect of the science. In the study of Nature, there is one conviction that grows ever deeper and more impressive as discovery succeeds discovery—the conviction that all material phenomena are one. In the world of Science there is nothing too mean to be infinite, nothing too brief to be eternal, nothing too abstract to be vital—above all, there is nothing common or unclean.

> "All things, by immortal power,
> Near or far,
> Hiddenly
> To each other linkéd are,
> That thou canst not stir a flower
> Without troubling of a star."

This is not merely Poetry—it is Science, it is Mathematics, it is Astrophysics. In the following pages we attempt to

trace the hidden threads that bind us to the stars. Some we believe we have found : others we feel sure are there. And it is not only the network of space that we seek : we follow also the subtler cords that stretch through time and make us one with the dawn of things. We have begun, from necessity, with the things of Earth : we reach out now to the Universe.

Section II
CHARACTERISTICS OF THE STARS

CHAPTER V

STELLAR SPECTRA

SPECTROSCOPY, in leaving the laboratory for the outer world of Space, undergoes no change of principle or of method. So far as the observations and their general interpretation are concerned, the only modification of the processes we have already described is that, instead of constructing our own sources of light, we take those which Nature has spread with so prodigal a hand in the heavens around us.

But there is, nevertheless, a new outlook. In the laboratory, the "unit spectrum," as we may call it, is the spectrum of a single element: in celestial spectroscopy, the unit is the spectrum of a single star or nebula. The classification of the material is therefore profoundly different in the two cases.

We may regard the two branches of spectroscopy as two aspects of the one science of Astrophysics. In the laboratory, the physical side of the subject is emphasised. The spectrum is brought into relation with the other properties of the element —its atomic weight, melting point, chemical properties. In stellar work, on the contrary, the bias is on the astronomical side. The spectrum of a star or nebula is considered in connection with the mass, density, luminosity, and other qualities of the same star or nebula. On the one hand, we have astronomical physics; on the other, physical astronomy.

Of course, the distinction is not absolute. As we have already pointed out, the physicist has as much to learn from the stars as the astronomer has from the laboratory; and, in the last analysis, the distinction between physicist and astronomer seems doomed to disappear. But that ideal state has not yet come. An atom is no more an ultimate physical unit than is a star, and in the present condition of science the distinction we have indicated is a very useful, and, indeed, a necessary one. We dealt in the last section with the main principles of astronomical physics: we are to turn now to physical astronomy, and see how the spectra of the stars enable us to augment, co-ordinate, and interpret the mass of facts which, by various means, have been brought to light.

A stellar spectroscope differs in no respect from the

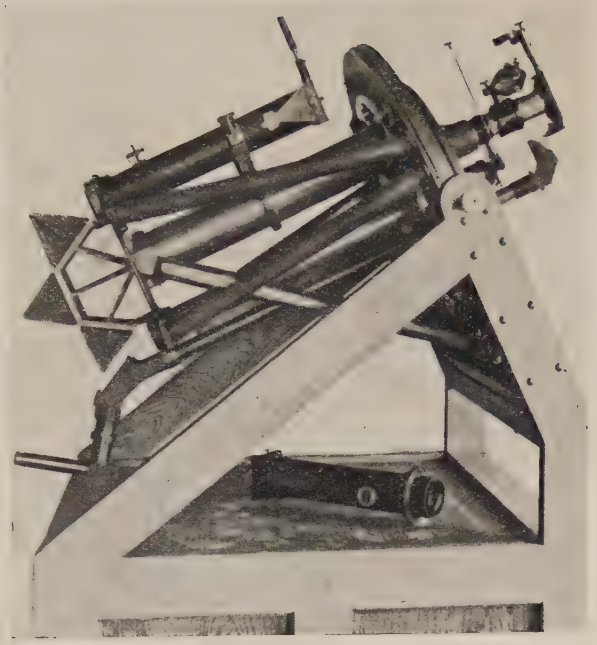
instruments we have described on an earlier page, except that its outward form is chosen so that it can be attached, with the greatest possible stability, to a telescope. The eyepiece of the telescope is removed, and the slit of the spectroscope is placed so as to fall across the image of the heavenly body formed by the object glass or mirror of the telescope. The light passes through the slit in the ordinary way, and the spectrum is photographed after an exposure which may occupy any length of time up to several hours.

It will be remembered that each line in the spectrum is an image of the slit. Since a star image is merely a point of light, the arrangement we have described allows only a single point of the slit to be illuminated, and so the resulting spectrum has no breadth. As this is not at all suitable for accurate work, the telescope is made to move in such a way that the image of the star travels slowly and steadily along the slit during the exposure. The spectrum then has a definite breadth, and a comparison spectrum from some terrestrial source can be taken on either side of it. For visual work the same result can be obtained by placing a cylindrical lens before the slit, so that the image of the star is drawn out into a line ; but the loss of light-intensity which is necessarily involved makes the device unsuitable for photographic observations.

Special precautions have to be taken to keep the temperature of the instrument quite uniform throughout the long exposures ; otherwise the results would be worthless for exact measurement. Arrangements have to be made also to ensure that the image of the star remains on the slit continuously, and that there is no disturbance while the operations are taking place. To obtain the best results, extreme skill and care are necessary. Plate XII. shows the Bruce spectrograph of the Yerkes Observatory. The essential points can be gathered without any further explanation.

The greatest difficulty of stellar spectroscopy is the scarcity of light. Not only are the star images which fall on the slit often very feeble in themselves, but the inevitable losses which the light suffers in passing through the spectroscope reduce it to a small fraction of the total amount that falls on the slit. The times of exposure are therefore sometimes extremely long, and, worse still, the spectra of stars below a certain limit of brightness cannot be obtained at all by this method. Unfortunately there is at present no more satisfactory way of obtaining stellar spectra suitable for measurement.

(*a*)



(*b*)

The Bruce Spectrograph of the Yerkes Observatory.

(*a*) Attached to 40-inch refractor. (*b*) Mounted on carriage with constant temperature case removed.

To face page 60.

For classification purposes, however, it is sufficient to obtain the spectrum without a terrestrial comparison. The arrangements can then be very much simplified. The slit and collimator are dispensed with, for the heavenly bodies are at such great distances from us that the portion of their light which reaches us is sensibly parallel already. A large prism, known as an "objective prism," is placed in front of the object-glass of the telescope, and the astronomical telescope itself fulfils the purpose also of the spectrograph telescope. The photographic plate is adjusted at its focus, and the spectrum is photographed. Breadth is given to the spectrum by allowing the telescope to move on its axis at nearly, but not quite, the same rate as the star appears to move in the sky in its journey westwards. The slight inequality in the rates of motion causes the star image to "trail" slightly on the plate, and the position of the prism is adjusted beforehand so that the length of the spectrum is at right angles to the direction of trailing.

The objective prism gives the spectra of stars which are much too faint for the slit spectrograph, and it has the additional advantage that the spectra of many stars can be obtained on the same plate by a single exposure. Mainly in this way the spectra of from two to three hundred thousand stars brighter than the twelfth "magnitude" (see Chap. VI.), have been photographed and classified. The objective prism, in fact, is the simplest possible form of stellar spectrograph, for the only addition to the ordinary telescope is the indispensable prism itself. There are two reasons, however, why the limit of brightness below which the spectrum of a star cannot be obtained seems destined to occur before the limit of brightness for direct photography. In the first place, it is impossible to avoid some loss of light in the prism, by absorption and reflection: and, in the second place, since the great majority of stellar spectra consist of dark lines on a bright background, the spectrum, in which the light is spread over a definite area, must always show less intensity of light in any part than the concentrated image of the star itself.

Of the general conclusions that have been drawn from the study of stellar spectra, we cannot in this chapter say very much: our whole book, in fact, is permeated by them. For the moment, we limit ourselves to the most obvious, and consider in detail only the principles on which the spectra have

been classified. And it is remarkable that the most obvious, the most superficial fact of stellar spectroscopy is, at the same time, perhaps the most impressive and significant: the fact, namely, of the almost universal uniformity of Nature. This reveals itself in two ways. First, the lines in celestial spectra show us that the same elements that we are familiar with on the Earth exist—in what a variety of circumstances we can hardly conceive—in stars throughout the sky; second, more than ninety-nine per cent. of the stars examined show absorption spectra, which, in the light of our laboratory experiments, assure us that the stars consist of an interior source of continuous spectrum surrounded by an atmosphere of cooler, but still luminous, vapours. Apparently Nature works with the same elements, and jumbles them together in much the same way, throughout the whole of the Universe.

On the complete significance of this tremendous fact, we can at present only speculate; while we await fuller knowledge, we can at least draw encouragement in our effort to read the secrets of the Universe in our own little corner. But, at the same time, lest we should be too confident, Nature throws in a few bodies which form the inevitable exception to what we might otherwise conceive to be the rule. It is perhaps not without a meaning of its own that the exceptions to the one piece of evidence of uniformity are closely associated with the exceptions to the other. There are a comparatively small number of stars and nebulæ whose spectra consist mainly or entirely of *bright* lines on an almost dark background, and it is just these spectra that contain the greatest proportion of lines which we have not yet succeeded in reproducing in our laboratories. We will not attempt here to discuss these things. Later, when we have covered more completely the field of our present knowledge, we shall be in a better position to see what their meaning might be.

The classification of stellar spectra has been attempted in various ways, but only two systems have survived; namely, those of Secchi and of the Harvard College observers. Secchi's classification, indeed, lives only because it is a convenient summary of the more complete Harvard system. Devised at a time when the data were very much more meagre than they are now, and based at least partly on different principles from those adopted at Harvard, Secchi's work, by the close

correspondence of its general features with those of the later observers, is no mean evidence of the foresight of its author.

The most striking fact that comes to light in classifications of this kind is that stellar spectra can be arranged in a sequence of types, the transition from one of which to the next is, in general, perfectly gradual. There are, relatively, very few stars indeed whose spectra stand outside this sequence. When this is recognised, the problem of classification is simply that of making the most convenient arbitrary divisions in the continuous succession of types.

Secchi, though he based his classification ultimately on the spectrum lines, was guided by the colours of the stars. He saw that stars of the same spectral type had the same colour, and he noticed a gradual change of colour in passing from one star to another in the order of the spectral sequence. He therefore divided the spectra into four "Types," characterised most simply by the colours of the stars represented in them. Type I. denoted the spectra of the whitish or bluish stars; Type II., those of the yellow stars; Type III., those of the orange or reddish stars; and Type IV., those of the very red stars. It must be remarked that the transition from Type III. to Type IV. shows some discontinuity, so far as the spectra are concerned, though the colour sequence seems, superficially, to be perfectly continuous.

The Harvard work was begun in 1885, as a memorial to the late Henry Draper: hence the term "Draper classification" is often used to indicate its results. The main criterion of this classification is the variation in the relative intensities of certain selected lines which is observed in passing along the sequence. Corresponding to the predominance of each set of lines, a letter is assigned, denoting a spectral type. In the course of time, certain divisions have been found to be unnecessary, and in some respects the original order of the sequence itself has been modified, so that the present Harvard divisions show only a trace of the alphabetical order in which the type symbols were originally assigned. As the arrangement stands now, more than ninety-nine per cent. of the spectra are included in the six divisions—*B*, *A*, *F*, *G*, *K*, *M*. Secchi's Type I. corresponds to *B*, *A*, and early *F* spectra; Type II., to later *F*, *G*, and *K*; and Type III., to *M*. Type IV.—which follows Type III., as we have said, with a slight discontinuity—is a small class represented by the letter *N*; and, more recently, another

small division of red stars has been recognised and given the letter *R*. In addition, there are bright-line stellar spectra, of which Secchi's classification took no account. These have been included in a class *O*. A class *Q* was formerly proposed for certain peculiar bright-line spectra. It has now dropped out of use, however, and the symbol has been suggested for the spectra of *novæ*, or new stars. The spectra of the gaseous nebulæ are included in the Harvard system, and denoted by the letter *P*. To make the story complete, it should be mentioned that a new class of red stars has recently been observed (*e.g.*, π^1 Gruis, *R* Cygni, *R* Andromedæ), which does not appear to fit in with the general Harvard classification. The spectra are very complicated : it has been proposed to denote them by the letter *S*.

It has not been easy to decide how the relatively small classes, *P*, *O*, *N*, *R*, *S*, should be placed in relation to the main sequence—*B* to *M*. It seems probable now that *O* precedes *B*, and that *P* is to be placed at the head of the list, immediately before *O*. The *R* and *N* spectra, however, appear to have no place in the sequence, and it has lately been suggested that they form a branch of the chief series, which they leave at a point between *G* and *K*. The *S* spectra probably lie along another branch, meeting the chief series at *K*. The present Harvard classification may therefore be written [1]—

P, *O*, *B*, *A*, *F*, *G*, *K*, *M*.
→ *R*, *N*.
→ *S*.

The Harvard criterion of relative intensity of lines is a delicate one, and allows of a considerable degree of subdivision of the main types. The symbolism has, therefore, been extended by the addition of small letters or figures after the capital letters. Thus, class *P* includes *Pa*, *Pb*, *Pc*, *Pd*, *Pe*, *Pf* ; class *O* runs from *Oa* to *Oe*, with *Oe* 5 denoting a transition stage between *Oe* and the earliest *B* spectra. From *B* to *K*, inclusive, the figures 0 to 9 are employed instead of small letters ; thus, the earliest *B* spectra are represented by *B*0, while *B*5 represents a stage half-way between *B*0 and *A*0. Class *R* also is subdivided in this way. In the *M* and *N* classes, there is a return to the small letters : *M* spectra comprise

[1] The spectra at the beginning of this sequence are sometimes referred to as "early type," and those at the end as "late type" spectra. The reader is warned not to draw conclusions from the choice of these terms. They are to be understood as symbols only, denoting the position of the spectrum in the spectral sequence.

Ma, Mb, Mc, Md, while class *N* extends from *Na* to *Nc.* The use of small letters instead of figures denotes that the subdivision of the types is not sufficiently well established to be regarded as final.[1]

It is important for our purpose that we should consider the general features of the different spectral classes. We shall see as we proceed that the spectrum is related so intimately to practically every other quality of a star, that it forms the natural basis of a classification of the stars themselves. We therefore make no apology for giving the spectral sequence rather detailed attention. At the same time, it must be understood that, in the following summary, certain points of technical detail have been omitted.

Class P [2] (Gaseous nebulæ). These spectra are all composed of a few sharp bright lines on a very faint continuous background. The chief lines are due to hydrogen (which occurs throughout) and ionised helium, and there are a few strong lines which have not been identified. Possibly they arise from one, or two, unknown elements. They include two green lines, whose wave-lengths, in angstrom units, are 5007.05 and 4959.02, and there is a close pair at 3728.84 and 3726.10. The second pair appears to have a different origin (or to be produced at its strongest under different conditions) from the green lines. In the division *Pa* (type—I.C. 418 [3]), the pair 3729-26 is most prominent, while the transition to *Pb* (type—Orion nebula) is marked by the increased intensity of the lines 5007, 4959. In *Pc* (type—I.C. 4997), an unknown line at 4363.4 and a hydrogen line ($H\gamma$) are the principal features. The green line at 5007 is specially developed in *Pd* (type—N.G.C. 6826), and likewise in *Pe* (type—N.G.C. 7662), but in the latter class appears also the ionised helium

[1] Since this chapter was written, it has been decided by an international committee on the classification of stellar spectra to convert the symbols *Ma, Mb, Mc,* into *Mo*, *M*3, *M*8 respectively, and to abandon the symbol *Md*; also, the symbols *Na* and *Nb* are to be changed to *No* and *N*3.

[2] The subdivisions given for this type have been proposed recently by Miss Cannon: they have not yet been definitely established, since further investigations appear to be necessary before the final classification is made.

[3] Except the brighter stars (which have names of their own, or take the genitive of their constellation name, preceded by a distinguishing Greek letter), the stars and nebulæ are indicated by their numbers in recognised catalogues. Thus, I.C. stands for *Index Catalogue*; N.G.C. for *New General Catalogue*; M. for *Messier's Catalogue*; B.D. for *Bonn Durchmusterung,* etc.

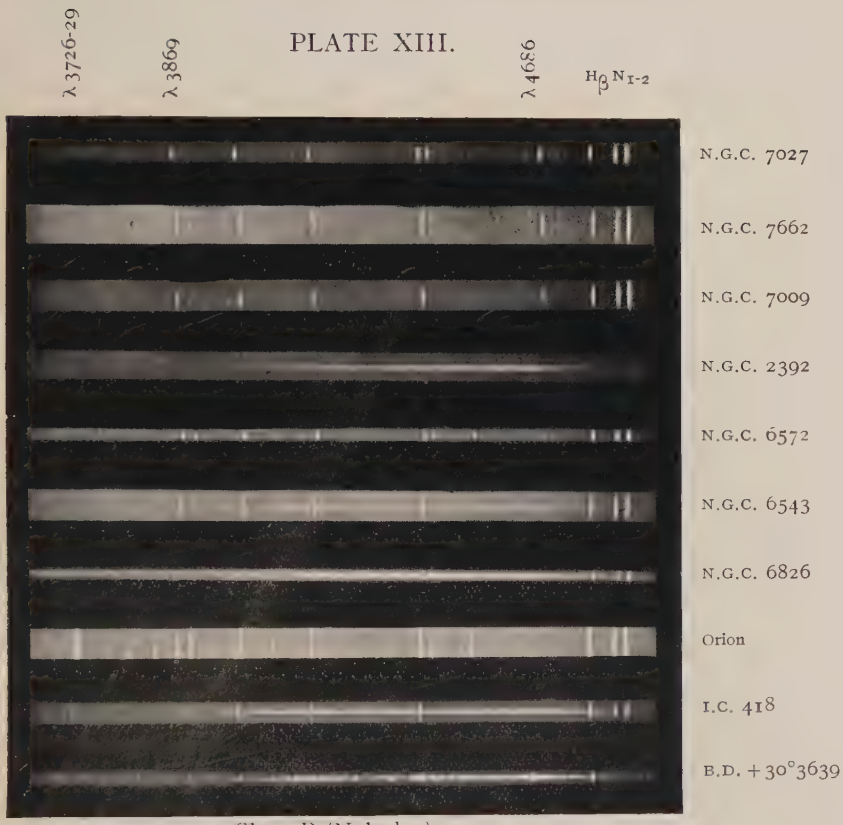
line at 4686, whose story we told in a former chapter. The majority of gaseous nebulæ belong to classes *Pd* and *Pe*. *Pf* (type—N.G.C. 40) is characterised by a bright band, or complex broad line, whose centre is at 4650. It is the presence of this band that links the spectral classes *P* and *O* with one another.

Class O. This class includes all the stellar spectra whose chief lines are bright. The stars giving rise to them are often called *Wolf-Rayet* stars, after the two observers who first called attention to their characteristics. The chief features are broad, bright lines in the blue and yellow regions, some arising from ionised carbon, nitrogen and oxygen, and others from origins which have yet to be discovered. There are also the bright, ubiquitous hydrogen lines, and the lines of neutral and ionised helium.[1] There is a continuous background to the spectrum, which is strongest in the ultra-violet. No metallic lines appear. The division *Oa* (type—γ Velorum) is dominated by the broad line at about 4650, which appears first in class *Pf*. Both bright and dark hydrogen lines occur also. *Ob* (type—B.D.+35°, 4001) shows bright lines of hydrogen and ionised helium. In *Oc* (type—B.D.+36°, 3987), the bright lines of *Ob* have become narrower, and there is also a bright band at 4638, of unknown origin : there are no absorption lines. *Od* (type—ζ Puppis—see Chap. IV.), has all its lines dark except 4686 and 4638. Hydrogen and ionised helium are shown. *Oe* (type—29 Canis Majoris) is similar, but there are many more dark lines, due chiefly to helium. A pair of lines at 4097.5, and 4089, due respectively to ionised nitrogen and ionised silicon, is prominent. In *Oe*5 (type—τ Canis Majoris), all the lines are dark. The presence, in considerable intensity, of lines due to both neutral and ionised helium, makes this type intermediate between the later *O* and earlier *B* spectra. The complex line at 4650 is still present, but now as an absorption line.

Class B. From this class onwards, the spectra—with a few special exceptions which will be mentioned—contain only absorption lines and bands on a continuous background.

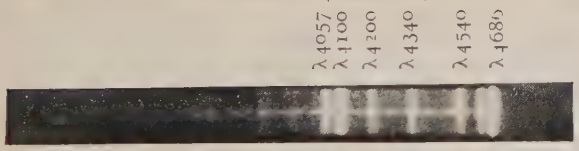
[1] From what we have already said, the reader will understand that helium is, for spectroscopic purposes, a different element in the ionised state from what it is when neutral. The respective lines occur, in general, under different conditions, and the mere statement that a spectrum shows "helium" is not sufficient for the complete elucidation of the circumstances of its origin. A similar remark applies, of course, to other elements.

PLATE XIII.



(a)

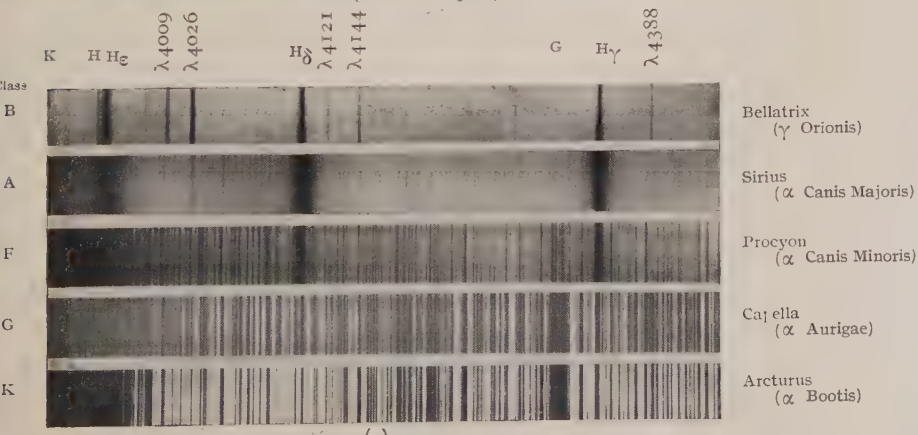
Class P (Nebular)



(b)

Photo: Max Wolf.

Class O (Wolf-Rayet)



(c)

Spectra of the Harvard Sequence (Earlier Types)

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In class *B*, the lines of hydrogen and neutral helium predominate. Ionised helium appears in the spectra of some of the stars, which seem to be associated with nebulosity. The *B* stars are often called "helium" or "Orion type" stars. Enhanced lines of silicon, oxygen, and nitrogen are prominent, and there are faint indications of a few metals. Both neutral and ionised magnesium appear. The stars of the Pleiades, Regulus, Rigel and other Orion stars are types. In *B*0 ionised helium appears faintly, while ionised oxygen and silicon are strong. In *B*2 neutral helium predominates over silicon and oxygen, which decrease in prominence rapidly as the divisions advance. In *B*5 occur lines of silicon corresponding to a lesser degree of ionisation than those already mentioned, and the *K* line of ionised calcium makes its appearance. Ionised magnesium is conspicuous. Neutral helium then fades out rapidly, until in *B*9 only one of its lines (4026) remains to distinguish the group from type *A*.

Class A. Class *A* spectra (*e.g.*, Sirius, Vega) are characterised by an enormous predominance of hydrogen lines. Helium has disappeared, or is scarcely visible. There are some fine, weak metallic lines, notably of ionised calcium and iron. In *A*0, ionised calcium and magnesium give the most prominent metallic lines, but there are many faint ones from other sources. The progression through the group is marked by the gradual weakening of hydrogen and the strengthening of the metallic lines. In *A*5 ionised magnesium has lost its relative importance among the latter, and iron, titanium, and calcium rise into prominence. But the hydrogen lines are by far the strongest feature throughout the class.

Class F. This may be called the calcium type, for the two strong *H* and *K* lines of ionised calcium are the strongest in the spectra. Hydrogen has degenerated, but is still conspicuous. The general metallic lines have gained in intensity compared with class *A*. In *F*0 (*e.g.*, Canopus), the group of lines known in the solar spectrum as the *G* group (consisting of a number of metallic lines, chiefly iron) is distinct, but weak. It strengthens consistently as the type advances, till, in *F*5 (*e.g.*, Procyon), we have a spectrum strongly suggestive of that of the Sun. Ionised iron, however, predominates over neutral iron in the *G* group. Neutral calcium gains in strength towards the later subdivisions. In *F*8 we have a spectrum which differs from the solar spectrum only in the relative intensities of the hydrogen and ionised calcium lines.

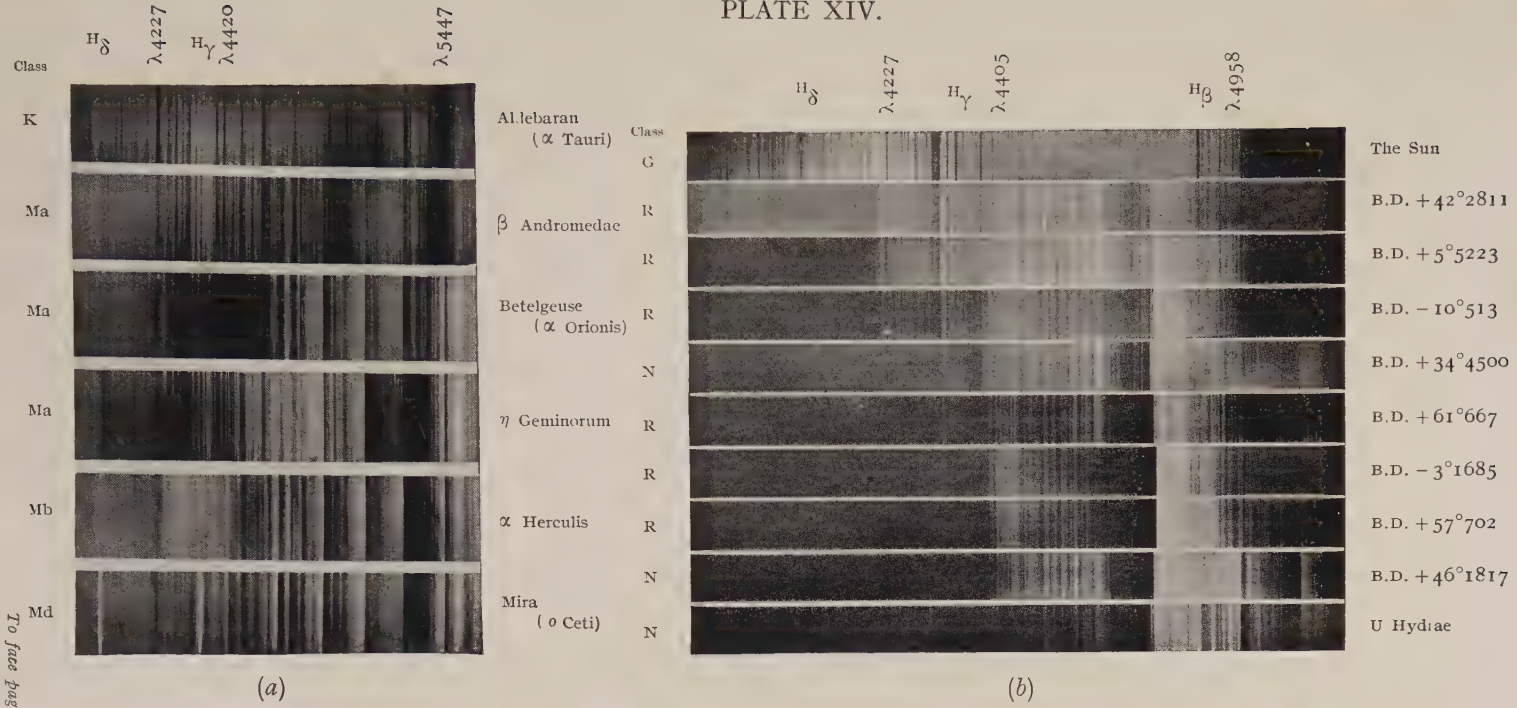
Class G. Class *G* includes the solar type of spectrum. It is distinguished, not by the predominance of the lines of one substance, but by multitudes of strong metallic lines, among which those of iron are the most conspicuous. Hydrogen and ionised calcium are still prominent, but the hydrogen lines are distinctly on the wane. Capella (*Go*) has a spectrum almost identical with that of the Sun.

Class K. The yellow stars, of which Arcturus is a type, show a continuance of the movement indicated in class *G*. Hydrogen is still less prominent, and the metallic lines are relatively stronger, while ionised calcium is still very pronounced. The *G* group remains conspicuous. In *K*5, however (type—Aldebaran), some of the components have disappeared, and a simplified structure of the group is shown. In the later divisions there are faint indications of bands of titanium oxide.

Class M. In Class *M* the titanium oxide bands reach their full strength and, in the later divisions, completely dominate the spectrum. In *Ma* (*e.g.*, Betelgeuse, Antares), a number of lines are still fairly prominent. The *G* group is still less complex, and the line 4227, of neutral calcium, is noticeably strong. The continuous spectrum has become very weak at the violet end, so that the *H* and *K* lines of ionised calcium (which belong to the violet region), though present, are hardly visible. In *Mb* neutral calcium and the titanium oxide bands are still more conspicuous, fresh bands having appeared in the green and yellow. In *Mc*, many of the lines have disappeared, owing probably to the masking effect of the strengthened bands. *Md* is a group composed entirely of a class of "variable" stars (see Chap. XV.), of which Mira (o Ceti) is the type. Bright lines appear again in this division—particularly one line of hydrogen (H_α), but sometimes other bright lines also. Apart from these, the spectrum is dominated by the bands of titanium oxide.

Class R. In one respect these spectra are not so far removed from the *K* class as are the spectra of Class *M*: their continuous background has not become so weak at the violet end. From another point of view, they suggest the spectra of Class *O*, and have been described as *O* spectra reversed; that is, showing dark lines on a bright background. The principal feature is a group of carbon bands, extending from 4736 to about 4600. In *Ro* (type—S.D. 10°, 5057), this group is conspicuous, as well as the line 4227 (neutral

PLATE XIV.



(*a*) (*b*)

Spectra of the Harvard Sequence (Later Types)

To face page 68.

calcium) and certain iron lines. Spectra intermediate between *K* and *R*o have lately been observed. The advance through the group is marked chiefly by a weakening of the continuous spectrum in the violet. *R*8 is with difficulty distinguished from *Na*.

Class N. These spectra contain dark bands due to carbon and its compounds, and a number of dark lines of hydrogen, magnesium, sodium, iron and other metals. There are also numerous bright lines, of which all except those of hydrogen are unidentified. The divisions of this group are distinguished by the relative intensity of the two portions of continuous background which are separated by the group of carbon bands which appears first in Class *R*. In *Na* (type—19 Piscium), the violet portion has $\frac{7}{10}$ths of the intensity of the red portion. In *Nb* (type—B.D.+67°, 350), the ratio has fallen to $\frac{5}{10}$ths, while in *Nc* (types—*VX* Andromedæ, *S* Cephei), the violet portion is practically invisible. Special photographic plates have to be used to photograph these stars. Secchi likened them to "drops of blood in the field of the telescope." It will be noticed that, in class *N*, there is a departure from the ordinary Harvard criterion of classifying spectra by means of their lines. It would be difficult, however, to carry out the ordinary methods with such special types of spectra as are those of this class.

Class S. This is a small class which has only lately been distinguished. Typical stars are π^1 Gruis, *R* Andromedæ, *R* Geminorum. The stars are red: their continuous spectra begin to weaken on the violet side of 4500, and degrade in intensity very rapidly after 4430. Titanium oxide bands appear in the spectra, but are not nearly so prominent as they are in the spectra of class *M*. Strontium and ionised barium show prominent absorption lines, and there are bright lines due to ionised iron. Most of the stars are "variable" (see Chap. XV.), and these show bright hydrogen lines. The most characteristic feature of the spectra is a "complicated structure" at 4630 – 4660, which apparently contains both absorption and emission lines. An absorption band in the red, with its head at 6470, is probably a characteristic of the class.[1]

[1]Merrill, acting on a suggestion of Baxandall's, has recently identified bands in *S*-type spectra with those of zirconium oxide. The three branches of the Harvard sequence thus show, respectively, oxides of titanium, carbon, and zirconium—elements belonging to the same chemical family.

The few spectra which do not fall exactly in any of the general classes are indicated by the type symbol of the class they most resemble, followed by the word "peculiar." A number of typical spectra are shown in Plates XIII., XIV., and XV.

The following table summarises the leading characteristics of the various types :—

TABLE I

SPECTRAL TYPE			CHARACTERISTICS	EXAMPLES
HARVARD	SECCHI	OTHER TITLES		
P		Nebular	Bright lines of hydrogen and ionised helium, and some of unknown origin.	Orion nebula.
O		Wolf-Rayet.	Bright lines of hydrogen and of ionised helium, carbon, nitrogen, and oxygen; some unknown bright lines.	γ Velorum
B		Orion; Helium.	Dark lines of hydrogen and neutral helium predominant; lines of ionised silicon, oxygen, nitrogen, magnesium, calcium.	β, γ, δ, ε Orionis, Spica, Regulus.
A	I.	Sirian.	Hydrogen predominant; relatively weak lines of ionised and neutral metals.	Sirius, Castor, Vega.
F	I.-II.	Procyonian.	Lines of hydrogen less prominent than in *A*; metallic lines stronger; ionised calcium very strong.	Procyon, Canopus.

PLATE XV.

Titanium Oxide Bands in M-type Spectra.

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TABLE I—*Continued.*

SPECTRAL TYPE			CHARACTERISTICS	EXAMPLES
HARVARD	SECCHI	OTHER TITLES		
G	II.	Solar.	Lines of neutral metals most conspicuous: some lines of ionised metals, notably calcium.	Sun, Capella.
K	II.	Arcturian.	Enhanced lines weaker and neutral lines stronger than in *G*.	Arcturus, Pollux, Aldebaran.
M	III.	Antarian.	Bands of titanium oxide; neutral lines of metals.	Antares, Mira, Betelgeuse, α Herculis.
R			Carbon bands; neutral lines of metals, particularly calcium.	S.D. 10° 5057.
N	IV.	Piscian.	Stronger carbon bands; weak violet end of spectrum.	19 Piscium, 152 Schjellerup.
S			Absorption bands —some of titanium oxide; dark lines of neutral metals; bright lines of ionised iron and sometimes hydrogen.	π^1 Gruis, *R* Andromedæ *R* Geminorum

From the foregoing account it appears that the successive stages in the advance of spectral type are the replacing of unknown lines by lines of ionised helium, of ionised helium by neutral helium, neutral helium by hydrogen and ionised

metals, these by neutral metals, and neutral metals by chemical compounds. From what we have seen of the conditions under which spectra are produced in the laboratory, we may at once draw the general conclusion that, with advancing spectral type, there is a continuous diminution in the vigour of the light-producing stimulus.

This follows not only from the supersession of enhanced lines by lines of neutral atoms : it is to be seen in the succession of enhanced lines themselves. Thus a very strong stimulus is necessary to ionise helium, a moderately strong stimulus will ionise magnesium, while calcium is ionised to a considerable degree even in the outer flame of an electric arc. Accordingly, we find ionised helium lines disappearing in the earliest *B* stars ; ionised magnesium remains until *G*o ; while ionised calcium reveals itself practically to the end of the sequence. It is tempting to argue from analogy that the unknown lines of the nebulæ and Wolf-Rayet stars originate from known sources which are still harder to stimulate than helium : that, however, is a matter which we shall leave for the present. But it is important that we should recognise, at the very outset, that the variation of spectral types is almost certainly to be attributed to a variation of the conditions of luminosity in the stars, and not to differences of chemical composition. Thus, the fact that *A*-type spectra are dominated by lines of hydrogen probably does not mean that the corresponding stars are mainly composed of hydrogen, but simply that the conditions existing in *A*-type stars favour the production of those hydrogen lines in preference to the lines with which we are familiar in the spectra of other elements. For this reason, if for no other, it is necessary to emphasise the fact that the Harvard classification is a classification of *spectra*, and not of *stars*. Fundamental as the spectrum is in the nature of a star, it is not the star itself. It must be taken in conjunction with the other stellar attributes before the full story can be unfolded.

We must be on our guard, also, against the idea that, even about the spectrum, the Harvard classification tells us all there is to be told. The spectrum would not be the great revealer it is if it told us no more about a star than that it belonged to a class containing thousands of other precisely similar stars. In classifying stellar spectra according to the relative intensities of conspicuous groups of lines, the Harvard observers have made what has proved to be a singularly happy

choice for their purpose: they have not, however, even approximately exhausted the details in which one spectrum may differ from another. The minute displacements of lines from their normal positions, the intrinsic characters of the lines, whether fine or broad, sharp or diffuse, symmetrical or unsymmetrical—these have been ignored in the final classification. This, no doubt, is as it should be, for simplicity is the first consideration in any classification of miscellaneous data. But to leave such characteristics out of account because they could not profitably be noticed in the general classification, would be fatal to even the crudest understanding of the sidereal universe.

For example, in the early days of the Harvard work, Miss Maury observed that the stars of the earlier spectral types could be separated into three parallel sequences (*a*, *b*, and *c*), differentiated by the degree of sharpness of their spectrum lines. Later research has shown that the *c*-stars (with very sharp lines), in particular, have a number of characteristics which distinguish them from the general body of stars of their own spectral classes. From the point of view of spectral classification, they still belong mainly to a larger group in class *A* ; in the consideration of the sidereal universe, they have a place of their own.

Again, even in the matter of relative intensities of lines, the Harvard criterion is not all-embracing. It applies to the strongest and most conspicuous lines in the spectra, but we know now that the intensities of less prominent lines, even in spectra of the same class, are the index to profound differences in luminosity of the stars.

In calling attention in this way to the limitations of the Harvard classification, let us repeat that we do not, in the least degree, underestimate the value of that classification. It is no exaggeration to say that it is the very backbone of the present body of Astrophysics. But we must recognise it for what it is. It is a classification of the characteristics of spectra which arise from particular physical conditions. What those conditions are, and how they are related to the other qualities of a star, are two of the main questions which we set ourselves, as far as may be, to answer.

CHAPTER VI

STELLAR MAGNITUDES

In order to reap the fullest advantage from the study of the general qualities of stars, we must consider those qualities in the light of the Harvard classification of spectra. In so doing we attack, at the same time, two problems: first, what physical conditions give rise to the spectra of stars? and, second, how are those conditions related to other stellar qualities? In the next few chapters, then, we propose to deal in turn with the various properties of the stars of which we can obtain more or less direct knowledge, showing how the data relating to each of them appear in the light of the Harvard classification. For simplicity, we will confine ourselves to the limited sequence, *B* to *M*, which contains all but a very small proportion of the spectra, leaving the small special classes for separate consideration at a later stage.

It is appropriate that we should begin with brightness—the feature in which the stars appear most obviously to differ. By brightness we mean—at present, at any rate—the *apparent* brightness of a star, which will be determined by the star's distance from us as well as by its intrinsic luminosity. We say, for example, that Sirius is brighter than Polaris, because it gives us more light; though, if we had no knowledge of the relative distances of these stars, we should not be able to say which would appear the brighter if they were placed side by side. Further, we assume that—except in certain special classes of "variable" stars, which we shall consider in Chapter XV.—the brightness of a star is constant. This is certainly not strictly true. Practically all the stars are in motion relative to us, so that the brightness is continually changing on account of change of distance. Again, we believe that the stars themselves are in process of development, which causes a more or less gradual change in the actual output of light. From at least two causes, therefore, the brightness of every star is a varying quantity. But these variations are infinitesimal and far beyond the reach of our measuring

instruments. The march of the large-scale cosmic processes is so inconceivably slow that—much more accurately than we can say of any terrestrial event—our observations are made at an instant of time. What we are about to consider, then, are effectively the constant apparent brightnesses of the stars.

One word more before we turn to the facts. The apparent brightness is not a measure of the total energy we receive from a star: it is a measure of our estimate of the energy. Thus, if there are two stars at the same distance, and one of them emits most of its energy in the invisible infra-red region of the spectrum, while the other has its maximum output in the blue, the latter will probably appear very much the brighter to our eyes than the former: it may, nevertheless, discharge a smaller amount of energy. In these days, we have more than one means of detecting light. We have the eye, of course, and we have various kinds of photographic plate. There are, accordingly, a number of possible scales of brightness. Three have come into general use, corresponding respectively to the estimates of luminosity by the eye, the ordinary photographic plate, and the isochromatic photographic plate. The ordinary plate is most sensitive to the far violet light, and is not affected at all by the red, so that a visually bright red star, like Betelgeuse, makes only a faint impression on it. The isochromatic plate is sensitive to all colours in varying degree, and the conditions of photography are chosen so that its records shall coincide as nearly as possible with those of the eye: it is intended, in fact, to give visual estimates of brightness in a manner independent of the personal peculiarities of the observer, and it has the additional merit of allowing the visual scale to be extended to stars too faint to be seen. It is necessary to employ a yellow light filter as well as the isochromatic plate to fulfil the conditions properly. The degrees of brightness of a star revealed by these three detecting agencies are expressed technically by what are known as the *visual*, *photographic*, and *photovisual magnitudes*.

The idea of magnitude is due originally to Hipparchus. Ptolemy employed it in his famous *Almagest*, and it has persisted to the present day. The twenty stars brightest to the naked eye were called stars of the 1st magnitude, while stars which were just bright enough to be seen were said to be of

the 6th magnitude. Stars of intermediate brightness were assigned intermediate magnitudes.

A more precise and extended form of this system is still in use. Ptolemy's 1st magnitude stars are, on the average, about 100 times as bright as those of the 6th magnitude. Definite standards of luminosity have been assigned to the terms "1st" and "6th" magnitude, such that the former is exactly 100 times as bright as the latter, and intermediate magnitudes have been defined in such a way that the ratio of brightness of stars of successive magnitudes is a constant quantity. It can readily be shown that this ratio must be 2.512: thus, stars of the 3rd magnitude are 2.512 times as bright as stars of the 4th magnitude, while these, in their turn, are 2.512 times as bright as stars of the 5th magnitude.[1] Allowance is made, of course, for absorption of light in the Earth's atmosphere, before the magnitude numbers are assigned.

The scale is extended to the fainter and brighter stars according to the same principles. Modern telescopes can reach stars of the 21st magnitude, while, for the brightest stars, zero and even negative magnitudes are introduced. The range of magnitude among the stars is enormous. From the foregoing principles, it follows that Sirius, of magnitude -1.6, is apparently more than a thousand million times as bright as the faintest stars that appear on modern photographs, and if we include the Sun, whose magnitude is -26.72, the range of apparent luminosity among the stars becomes too large to quote.

The conception of magnitude is not an entirely satisfactory one. It has the great disadvantage that the brightest stars are represented by the smallest—even zero and negative—numbers. When we speak of "stars of increasing magnitude," we mean "stars of decreasing brightness." This is not a natural mode of thought, and the reader who is not familiar with this nomenclature is advised to be on his guard against misinterpretation of statements which he may meet with, here or elsewhere, in which it is employed. In spite of its defects, it will continue in general use, because it appears universally in the vast collection of astronomical literature which now exists, and the confusion which would be caused by a change at this time would greatly outweigh the advantages.

[1] Generally, if L_m and L_n represent the brightnesses of stars of magnitudes m and n respectively, then $L_m/L = 2.512^{n-m}$.

The practical standards of luminosity are a collection of about 100 stars near the north celestial pole, known as the "north polar sequence." Owing to their position in the sky, these stars appear to remain practically at rest—a great advantage, for they are always at hand in the northern hemisphere, and are always observed through approximately the same thickness of air at any one station. Their magnitudes, extending from about 2.5 to 20 on the photographic scale, with intervals of about 0.2 magnitude, have been determined very exactly both photographically and visually, and new measures of magnitude are made by direct or indirect comparison with them. Since there is no direct means of comparing the magnitudes of a star on the photographic and visual scales, the "zero point" of each scale has to be fixed separately. We have seen how the visual scale has been defined—by choosing a definite standard of luminosity for the 1st or 6th magnitude, and a brightness ratio for successive magnitudes. There is an international convention that the photographic scale shall be constructed on precisely the same principles. The ratio is taken as 2.512, as before, and the 6th magnitude is defined so that it agrees with the 6th visual magnitude for stars of spectral type, *A*0. A star of this type, which is just visible to the normal eye, will therefore have the same photographic and visual magnitude, namely, 6. The brightness ratio is then all that is required to fix the magnitude of any star on either scale, according to its appearance to the eye or the camera.

The determination of stellar magnitudes is carried out by means of "photometers"—instruments by means of which the light from two sources can be directly compared. All practical visual stellar photometers act on the following principle. Rays of light from the two sources are brought into juxtaposition, and those from the brighter source are cut down to a known proportion until the intensities appear equal. The proportion in which the brighter has been diminished then gives the ratio of brightness.

Various instruments differ from one another in the nature of the comparison source of light, and in the means of reducing the intensity of the brighter. In the Potsdam instrument an artificial terrestrial comparison source is used, and equality of brightness is obtained by making use of the phenomenon of "polarisation" of light. The instrument includes a

pair of "Nicol prisms," which can be adjusted so that any desired fraction of the brighter source is transmitted, and, by a slight modification, this fraction can be selected so that it agrees in colour, as nearly as possible, with the light with which it is to be compared. The artificial source is used only as an intermediary. The north polar sequence provides the actual standards, and the magnitude of any other star is determined by comparing it and a standard star in turn with the artificial source. At Harvard, the stars themselves are the only sources used: a star to be measured is compared directly with the most suitable star of the north polar sequence. Two methods are adopted for equalising the light from the two sources. In one instrument there is a polarisation arrangement, similar in principle to the Potsdam device; in the other there is a graduated wedge of semi-opaque material, any desired thickness of which can be placed before the brighter star to reduce it to equality with the other.

Photographic magnitudes are measured in a number of ways. The most general method is to compare the brightness of two stars by the relative sizes of the images they form on the same photographic plate. A mathematical relation between the diameter of the image and the magnitude of the star is derived,[1] and the magnitudes are then calculated directly from the measured diameters, suitable corrections being made for the distance of the image from the centre of the plate. Another convenient method is to photograph the star image "out of focus," when the light is spread out into a circle, whose size depends on the distance from the focus at which the photograph is taken. The amount by which the star image has to be thrown out of focus to give the same *intensity* of image as the light of some convenient standard, is a measure of the magnitude of the star. For the determination of photovisual magnitudes, the only modifications of these methods are the substitution of an isochromatic for an ordinary photographic plate, and the introduction of a yellow colour filter.

[1] The equation is of the form—Magnitude $(m) = a - b.f(D)$, where D is the diameter of the image, and a and b are constants, the former depending on the length of exposure and the state of the atmosphere, and the latter on the instrument and plate used. $f(D)$ has been given various forms, *e.g.* $\log D$, $\sqrt{D}$, $D^{0.9}$, etc. For values of D between 0.05 and 0.30 mm., Hertzsprung gives the relation, $m = a - 34D + 35D^2$. Since comparison with standards is always made, the actual form of the function is not of great importance.

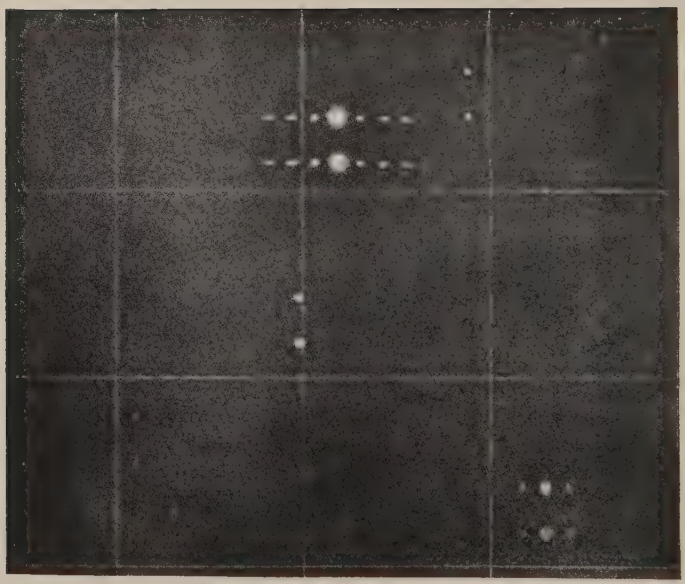

Fig. 7.—Star Images obtained with Coarse Wire Grating.

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Yet another method is to place a coarse wire grating in front of the object-glass of the telescope. The images of bright stars are then accompanied by one or more spectra, of very small dispersion, on either side. The spectra themselves (see Fig. 7) are almost points of light, and their intensities diminish with distance from the central image in proportions which are exactly known. Hence, if the central image of one star gives the same light-intensity as a spectrum image of another, the ratio of brightness of the two stars can at once be calculated. The grating is of very much coarser construction than the diffraction grating already described (p. 8), though its principle of action is precisely the same. Thus, Hertzsprung used a grating of overspun rubber cords, 3 mm. thick, separated by free spaces of the same width.

Within recent years, two much more sensitive photometers have come into use, largely through the researches of Stebbins. Strictly speaking, they establish a new scale of magnitude. Whereas the older instruments measure the visual or photographic effects of light, the new photometers depend on the electrical effects. There is no advantage, however, in accumulating scales. Those we have already considered are both of great value, for, taken together, they afford a very satisfactory estimate of the colours of the stars (see Chap. IX.). But there is no use for a "photo-electric" scale at present, and the indications of the new photometers are therefore interpreted, in accordance with experiments made for the purpose, in terms of the existing scales.

The earlier of the two instruments—the selenium photometer—depends on the curious property of the element selenium, by which it is a non-conductor of electricity in the dark, but becomes a conductor when light falls on it. The magnitude of the effect depends on the intensity of the light, so that, by allowing the light of a star to fall on a suitably constructed selenium cell and measuring the current of electricity produced in a circuit of which the cell forms part, we have all the data necessary for determining the star's magnitude. The second instrument, which promises to be still more useful, employs the phenomenon of "photo-electricity"—the release of electrons from certain metallic surfaces by the action of light. The number of electrons released is proportional to the intensity of the light, and since there are very accurate means of determining this number, we have all the essential features of a practical photometer.

The sensitiveness of these instruments is very great. They have already detected minute changes of magnitude that were hidden from the older methods, and there is every indication that the photo-electric photometer, in particular, will make very important contributions to our knowledge of the Universe as a whole. It is capable of measuring the brightness of a star with an accuracy of one-thousandth of a magnitude.

As a source of information about the individual qualities of stars, magnitudes are subject to a serious limitation. They are determined, not only by the actual brightnesses, which are intrinsic properties of the stars, but also by the distances of the stars, which depend on the totally irrelevant position of the Earth in space. Until fairly recently these factors could be separated only for a few stars, but with the greatly increased knowledge of stellar distances which the last few years have brought forth, it is possible now to compare the actual brightnesses of a large number of stars.

The apparent brightness of a constant source of light is inversely proportional to the square of the distance of the source from the observer, supposing none of the light is absorbed in the medium through which it travels. Consequently, when the distances of a number of stars are known, it is a simple matter to calculate what their magnitudes would be if they were all placed at the same specified distance from us. Magnitudes calculated in this way express the absolute brightnesses of stars, freed from the factor of distance. A standard distance of ten parsecs [1] has been chosen, and the magnitude which a star would have if it were placed at this distance is called its *absolute* magnitude. The validity of absolute magnitude as a measure of intrinsic brightness depends, of course, on the assumption that the light of a star suffers no absorption in its journey through space. There is evidence that general absorption is at least negligible in amount, so that the absolute magnitude may be taken to represent a real property of a

[1] A *parsec* is a unit of distance whose derivation will be explained in Chap. VIII. It is equal to about nineteen million million miles. The absolute magnitude (M) is calculated from the apparent magnitude (m) of a star whose *parallax* (see Chap. VIII.) is π, by means of the equation—$M = m + 5 + 5 \log_{10}\pi$. Some writers have chosen 1 parsec as the standard distance for determining absolute magnitudes. With this convention, the equation becomes $M = m + 5 \log_{10}\pi$. The former definition of absolute magnitude, however, is the one in most general use.

PLATE XVI.

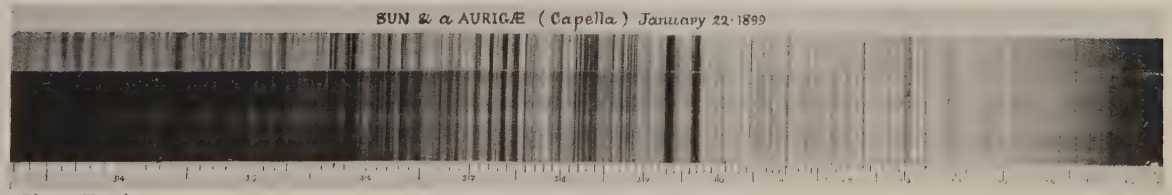

Photo: Huggins.

Spectra (G-type) of Giant and Dwarf Stars.

(*a*) Capella—Giant ; (*b*) The Sun—Dwarf.

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star. To avoid confusion, we shall henceforth speak of the magnitude measured by a photometer as the *apparent* magnitude, in contrast with the *absolute* magnitude just defined. It should be mentioned that the absolute magnitude is, as a rule, calculated from the photovisual scale.

It would seem that the determination of absolute magnitudes is entirely dependent on the measurement of stellar distances. Certainly it would be so if it could be approached only through the medium of apparent magnitudes. But lately a remarkable method of reading absolute magnitudes direct from spectra has been discovered and perfected by Adams and Kohlschutter at the Mount Wilson Observatory. It appears that the relative intensity of the members of certain pairs of lines in the spectrum of a star depends, though perhaps indirectly, only on the absolute magnitude of the star. It is sufficient, therefore, merely to measure the relative intensity in such pairs in order to obtain the absolute magnitude. Taking a number of stars whose absolute magnitudes were known from the apparent magnitudes and the distances, Adams and Kohlschutter found that a smooth curve could be drawn, connecting the relative intensities in each pair of lines in question with the absolute magnitudes : a separate curve was drawn for each pair. From these curves, the absolute magnitudes of large numbers of other stars were found : the pairs of lines selected for general use were—(*a*) 4216 (ionised strontium) and 4250 (iron) ; (*b*) 4455 (calcium) and 4462 (iron and manganese) ; (*c*) 4455 (calcium) and 4495 (ionised iron). Thus, (*a*) = −2 would imply that 4216 was two units fainter than 4250. For certain particular types of star, other pairs were found more suitable. The absolute magnitudes given by the three pairs were in excellent agreement. It should be noted that, even in stars of the same spectral type, wide differences of relative intensity were sometimes found : the lines affected are not the prominent ones chosen by the Harvard observers in the classification of the spectra on the basis of line intensity. Plate XVI. illustrates the effect of which we are speaking. It will be seen that, although the spectra of the Sun and Capella are identical in their general features, there are certain lines which show marked differences in intensity in the two spectra : thus, λλ4216, 4078 (ionised strontium) are stronger in the spectrum of Capella, while λλ4250, 4071 (neutral iron) are stronger in the solar spectrum. Capella is intrinsically much brighter than the Sun. It is

interesting to find that, in general, a pair of lines showing the effect contains one neutral and one enhanced line, and that the greater the absolute brightness of a star, the stronger is the enhanced line compared with the other. What the meaning of this might be we shall discuss later.

The measurement of the relative intensities has been carried out by Adams by means of a " spectro-comparator " —an instrument by means of which the images of two spectrum lines can be brought into coincidence. Major Lockyer, at the Norman Lockyer Observatory at Sidmouth, employs a wedge of neutral-tinted, semi-transparent material, whose thickness at the position where it makes the line just invisible when placed over it, indicates the intensity of the line. Absolute magnitudes are being measured now at a rate undreamed of a few years ago.

Adams and Kohlschutter noticed also two other influences of the absolute magnitude on the spectrum of a star—first, stars of great absolute brightness have abnormally strong hydrogen lines ; and, second, the spectra of these stars are relatively weaker in the violet region than are the spectra of the fainter stars. Neither of these criteria was found to be so reliable as the one adopted for absolute magnitude determination, but, within the last two years, Lindblad, working at Mount Wilson, has found it possible to measure the absolute magnitudes of *B* and *A* stars from the relative intensities of the violet and red portions of the spectra. It is for these stars that the method developed by Adams and Kohlschutter is least perfect.

Adams and Joy, however, have recently found that a more sensitive criterion of absolute magnitude for the *A* stars is afforded by the relative intensities of the neutral metallic lines in the spectra. They have effectively subdivided the spectral class afresh on this basis, and it appears that the absolute magnitudes of the stars are very definitely related to the spectral types thus defined. Their system of subdivision does not differ greatly in its results from that of the Harvard observers, so that there is very little error in the statement that, within class *A*, a knowledge of the spectral type of a star carries with it a knowledge of the absolute magnitude. There is evidence also that the sharpness or diffuseness of the spectrum lines may give a useful indication of the absolute magnitudes of exceptionally bright or exceptionally faint stars of this class.

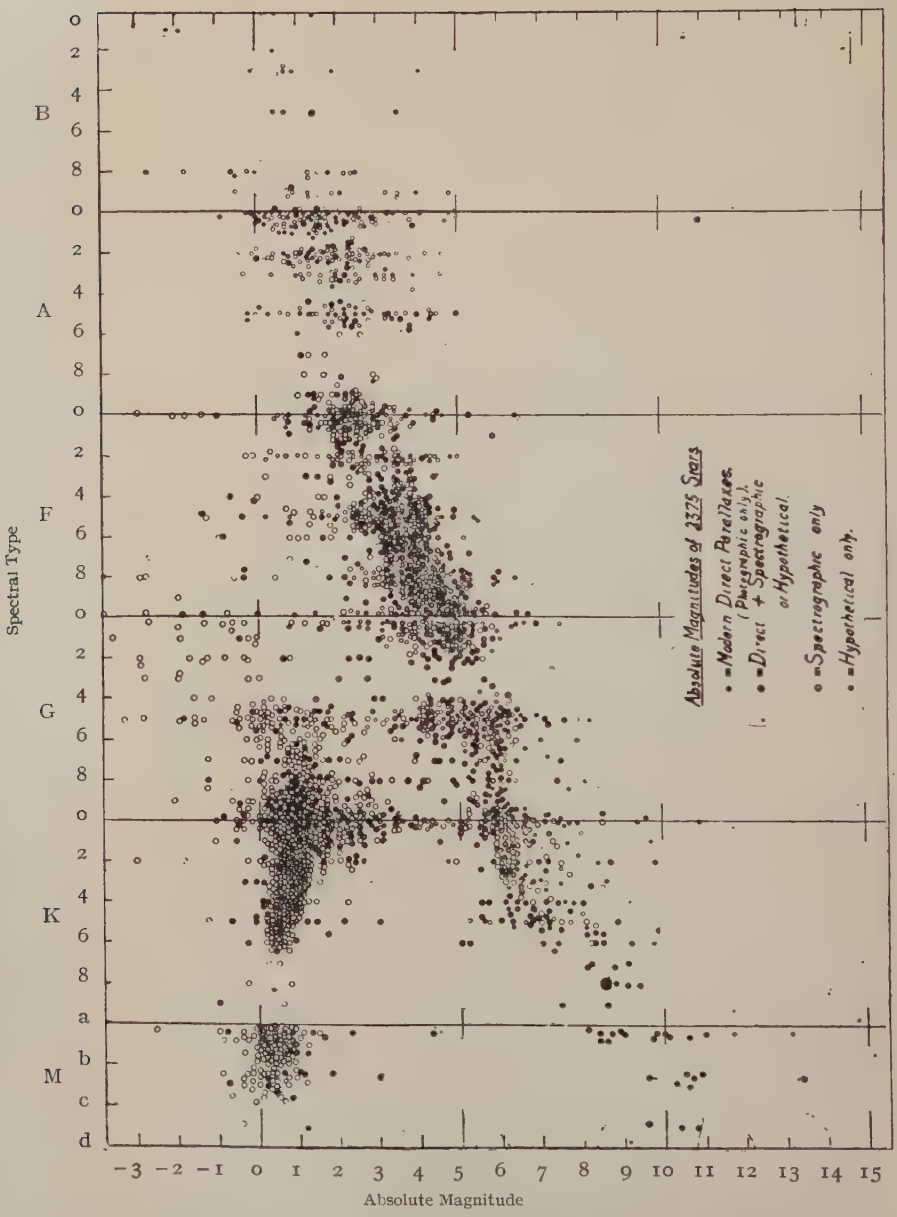

Fig. 8.—Diagram showing relation between Absolute Magnitudes and Spectral Types of Stars.

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We should not expect such large variations in the absolute magnitudes of the stars as we find in their apparent magnitudes. It seems, nevertheless, that the range of absolute magnitude is remarkably great, even if we omit the totally dark stars, which almost certainly exist. The star *A*0*e* (*N*) 17415 is only 0.004 times as bright as the Sun, while Canopus must be about 10,000 times as bright. This indicates a range of 16 magnitudes, and there are stars outside these limits. It seems that, so far as total luminosity is concerned, we have a great diversity of conditions to deal with in the stars, and we turn naturally to the relation between magnitude and spectral type as the first step in their investigation. Clearly, it will be more interesting for this purpose to deal with the absolute magnitudes than with the apparent ones, for they depend only on inherent stellar qualities.

Parenthetically, it should be mentioned that it has lately become possible to compare the total amounts of energy radiated by stars of the various spectral types. The total radiation is evidently a quantity of more fundamental importance than the visual or photographic magnitude, because it is independent of the properties of a particular recording instrument, such as the eye or a photographic plate. It defines what is called the *bolometric* magnitude, and is measured by the amount of heat into which the radiation from the star can be transformed. The results which have so far been obtained indicate that, if stars of the same visual absolute magnitude be compared, the total radiation is greater the later the spectral type.

The relationship between absolute magnitude and spectral type is a very remarkable one. On Fig. 8[1] each star is represented by a dot placed in the position corresponding to its condition with regard to these two qualities. The outstanding feature of this diagram is that the dots are clustered about two directions. They start from a common point among the *B* stars—which are all of high luminosity—and proceed, one along a line of almost constant magnitude, and the other along a line of regularly increasing magnitude (*i.e.* decreasing brightness). Thus, type *M* appears to consist of two groups, one of very bright and the other of very faint stars, with scarcely a star of intermediate magnitude. The same thing is true of the other spectral classes, but the division between the two groups becomes gradually

[1] Adapted from a diagram originally given by Curtis.

less marked as we proceed to the earlier spectral types until, in classes *B* and *A*, it practically disappears. The existence of these two groups among the *M* stars was first pointed out by Hertzsprung, who named the stars "giants" and "dwarfs," respectively, but it was Russell who showed that the division was to be found in the earlier types also, and who insisted on its importance when the data were much more scanty than they are now. We would point out that, at present, the terms "giant" and "dwarf" are to be taken as an indication of degree of luminosity only, and not necessarily of mass, volume, or any other quality. It will be our business later to discuss the cause of this very curious division, but for the present we make no statement concerning it.[1]

At the very beginning, then, we have come across strong evidence that the Harvard classification of spectra is not a classification of stars and therefore of all their properties. Here are two groups of stars, having identical spectra according to the Harvard criterion, whose luminosities are widely different. Whatever may be the primary cause underlying the general spectral variation of stars, it is certainly not total luminosity. On the other hand, however, there is evidently a very definite connection of some kind between luminosity and spectral type, or the dots would not be so regularly distributed in the diagram. To understand what that connection is, and what other factors enter into the problem, we must go farther afield.

[1]It will be noticed that there is a marked scarcity of giants of *F* and late *A* types. Reference will be made again to this curious fact. The scarcity of dwarf *M* stars is probably due merely to their faintness: only the relatively near ones come within range of our telescopes.

CHAPTER VII

STELLAR MOVEMENTS AND VELOCITIES

THE question of stellar movements is perhaps the most difficult in the whole range of Astrophysics. It has given rise in recent years to a large mass of literature, in which the names of Boss, Campbell, Dyson, Eddington, and Kapteyn figure prominently. The difficulty is not only that the measurements involved are exceedingly minute: the very meaning of motion, when the field of study is Space itself, needs re-definition. We know that all motion is relative. The statement that a body is moving implies that there is some standard of rest with respect to which it is moving. What standard of rest is there in a universe of stars?

It is the growing belief of men of science that Nature has provided no absolute standard of rest: we have to choose one for ourselves. In the early days of intellectual history, when our own globe was, for the most part, unknown, and ideas of cosmology were of the most elementary kind, the demands of philosophy were met by the assumption that the Earth was at rest. But growing knowledge of the changing positions of the planets strained this conception to the breaking-point. A new idea was necessary. It was not until the hub of the Universe was transferred to the Sun that the laws of planetary motion became intelligible. Then followed Newton's great conception of gravitation, and the solar system became a harmonised whole instead of an infinitely complex puzzle. Within the last fifty years or so, the new science of Astrophysics has arisen, to whose large vision the solar system is but a point among millions of others. What about the standard of rest now? Is it not just as misleading—nay, far more so—to choose the Sun as the standard for the Universe, as it was to put the Earth at the centre of the solar system? We must form a larger conception if we are ever to attain to a mental grasp of the Universe.

An extended body is capable of two distinct types of movement: it may travel as a whole in a certain direction, or it may rotate about an axis. Technically speaking, the motion

may be " translational " or " rotational "—or, of course, both together. A body has a rotational motion when its parts are moving with respect to fixed directions : it has a translational motion when its distance from any part of a fixed frame of reference is changing. Thus, a horizontal disc spinning in a room is said to have a rotational motion, because a radius is continually changing its direction with respect to the length of the room. A falling raindrop is said to have a translational motion, because it is changing its position with respect to the Earth considered as a fixed reference body. Before either type of motion can be detected—or, indeed, has any meaning at all—fixed directions and a fixed frame of reference must be defined. Our problem is to define these standards so that the motions of the stars with respect to them shall be of the simplest possible kind.

The direction of the Earth's axis seems to be a natural choice to begin with. Since three-dimensional space demands three directions in order to make possible a description of all movements in it, we might supplement this by choosing, say, two mutually perpendicular radii of the Earth lying in the plane of the equator, and suppose that they are at rest *in space* when they are rotating in that plane, *with respect to the Earth*, at the rate of one revolution per day. These are the three directions we take as fixed when we say—as any elementary text-book of Astronomy does—that the Earth rotates on an axis, whose direction is fixed, once every twenty-four hours.

But these directions do not quite satisfy our requirements. It is not that they have been defined with respect to the Earth, and therefore imply that the Earth in some way determines the motion of the Universe. Inasmuch as they are chosen as *directions* only, we might substitute any lines parallel to the Earth's axis and the chosen radii, which intersect, if we like, in the Milky Way—and still have made precisely the same choice. They do not involve any important assumptions with respect to the Earth : the Earth has been used for convenience in defining them, but they themselves are independent of it. The trouble is not that : it is that, if we make this choice, observations that can be made in a short period of time show that the stars will have to be regarded as performing revolutions of a very complex kind. The choice is unsatisfactory because it leads to too much complexity in the statements of the motions of the stars.

But if we suppose that the direction of the Earth's axis traces out in space a sort of wavy cone, having an angle of about $23\frac{1}{2}$ degrees (or, in other words, that the north celestial pole describes a wavy circle of this mean radius), and then tilt our three chosen directions as a whole, so that one of them points along the axis of this cone (or, to the centre of the polar circle), we shall choose standard directions which, so far as we know, will give us the greatest possible simplicity in our statements of motion. The elementary text-books tacitly change to these directions when they modify their earlier statement about the constancy of direction of the Earth's axis by saying that, on account of the "precession of the equinoxes," the north celestial pole describes a circle in the heavens, on which is imposed a wave motion which they call "nutation."

So much for fixed directions: there remains to be chosen a fixed reference body, so that motions of translation can be detected and measured. We may suppose three infinitely long rods to lie in the three directions we have mentioned, and take the structure they form as our body of reference. The question then arises: how shall motion with respect to this frame be defined? For example, if we suppose it fixed in the Sun, and then choose it as a standard of rest, we have effectively chosen the Sun as the standard, so far as translational motion is concerned. If, on the other hand, we suppose it fixed in Arcturus, which is moving rapidly with respect to the Sun, then we have chosen Arcturus as the standard of rest. The statements of the velocities of the stars will obviously be very different in the two cases. We have to decide, therefore, to what body, if any, we shall attach our reference frame, so that the resulting statements of stellar motions shall permit of the simplest kind of generalisation.

In matters of this kind, the astronomer always has one eye on the possibility that some day we shall learn how to interpret the movements of the stars, just as we now do the movements of the planets. It is natural, therefore, that he should choose a fixed reference body for the sidereal system according to the principles that have been found so satisfactory for the solar system. In our own family of planets, we take the Sun—or, strictly speaking, the centre of gravity of the whole system—as the standard of rest. On the assumption that gravitation is the principal force at work among the

stars, the standard of rest chosen for the stellar system is the centre of gravity of the Universe. We may say that the three mutually perpendicular rods, which constitute our reference body, are chosen so as always to intersect at this point.

The stars, then, are moving if they change their positions with respect to three imaginary mutually perpendicular rods, intersecting at the centre of gravity of the Universe. We can form no better conception of motion than this. But our difficulties are by no means over. The question now arises: how are we to measure velocities with respect to this standard? To determine the centre of gravity of the Universe we should have to know the mass and position of every piece of matter that exists, which we certainly do not know and have no hope of knowing. Our standard is, no doubt, an excellent one, but is is quite beyond our reach as a practical reality.

The only thing to do is to realise it as nearly as possible. We know much more about the positions of the stars than we do about their masses. There seems to be a kind of symmetry about the shape of the Universe. If this symmetry extends also to the distribution of mass, then the "centre of mean position" will serve our purpose as well as the centre of gravity, and, indeed, the two points will coincide if the symmetry of both kinds is perfect. We substitute, then, the centre of mean position for the centre of gravity.

But, even now, the problem is not settled. We believe that the gravitational influence of a body extends throughout the whole of space: there is no limit of distance beyond which it ceases to exist. Consequently, the centre of gravity of the Universe is not fixed until all gravitating matter is taken into account. But we do not know the positions of all the gravitating matter. We know only the bodies which send us sufficient radiation to impress our eyes or our instruments, or are revealed to us by the obstruction of such radiation. Consequently, although the centre of mean position of the whole of the stars might be a satisfactory substitute for the centre of gravity, it by no means follows that the centre of mean position of the limited number which we can photograph is equally satisfactory. This difficulty we cannot—as yet, at any rate—overcome. We have to be satisfied with the stars we know.

And not all the stars we know—or even the majority of them—are available for our purpose. Not only is their number so great as to make the task hopelessly large, but

by far the greater number of them are apparently so faint, or are so distant, that we cannot obtain evidence of their motion at all. We are limited, in the end, to the stars which are nearest to us or are bright enough to make velocity measurements possible.

What is done, then, is, in principle, this. We are compelled to use the Earth as the standard of rest for our actual measurements, because we cannot leave it. Since, however, we know the motion of the Earth with respect to the Sun, we can reduce all our measurements to the Sun as the standard. We have then a number of measurements of stellar velocities referred to the Sun as a rest-system. Suppose all these velocity measurements are resolved, and summed up, along the three fundamental directions. We shall not expect them all to cancel out: probably there will be a resultant velocity along each direction, and these resultants can be combined into a single resultant, having a perfectly definite value, along a new direction. Suppose the magnitude of this final resultant is V, in a direction which we may denote by θ. Then our result means that, taking the Sun as a standard of rest, the mean velocity of all the stars considered is V, in the direction θ. But we have agreed to regard the mean position of all the stars considered as a position of rest—or, in other words, to regard the mean velocity of all these stars as zero. We can reconcile this convention with our measurements only by supposing that the Sun itself is moving, with velocity V, in the direction opposite to θ.

From this point of view, measurements of stellar velocities are the data from which we determine the motion of our own solar system through space. But they are more than that. Having determined the solar motion, we can reduce our measurements of the velocities of other stars to the standard reference body, and so obtain a knowledge of the motion of each star in our field of inquiry with respect to the conventional standard. This is what we agree to call the *true* motion of a star. Evidently, we do not know the motion of any star until we have determined the motion of all—or, at least, of the number which we are forced tocons ider as representative of all. Taken as a whole, the velocities will, of course, neutralise each other, and tell us nothing, but groups of stars may yet show special types of motion with respect to the remainder, which may yield valuable information about the structure and operations of the Universe. We shall

consider stellar motions, then, with two objects in view—namely, the determination of the solar motion, and the investigation of possible systematic motions common to a group or groups of stars. Afterwards, we will see if we can find any connection between stellar velocities and spectral types.

The preceding discussion shows that we have to fall far short of our ideal of measuring stellar motions with respect to the centre of gravity of the Universe: the difficulties appear to be unsurmountable. Step by step we are forced back to the approximation, the principle of which we have just described. We have now to point out that, in the practical problem, even that approximation could not be carried out in its direct form until within the last year or two. The reason is that we have no means of measuring the velocity

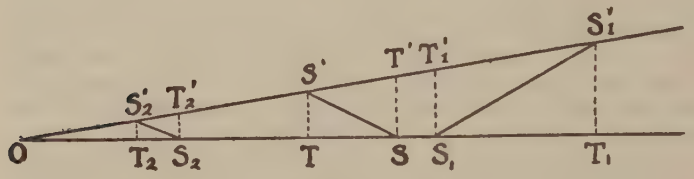


Fig. 9.

of a star at one step. We have to discriminate between the component of motion in the line of sight and the component of motion across the line of sight, and to determine them separately and by totally different methods. And since the groups of stars for which these two components are measurable are not identical, we should have had seriously to limit our already scanty information, in a special way, if we had confined ourselves to the stars for which we knew both of them, and therefore the complete velocity. It has accordingly been the custom to treat the two components of motion separately, and to assume that the motions of the stars are sufficiently at random to enable the solar motion to be determined from either of them. Before entering on the modified problem which thus arises, we must see how the two components of motion are defined and measured.

Suppose that, in the year 1870, a star was at the point S (Fig. 9), and that, by the year 1920, it had moved to S'.

Let *O* represent the observer, supposed to be stationed on the Sun. We can suppose that the star covered the distance *SS'* in the two steps, *ST'*, *T'S'*, where *ST'* is part of a circle with *O* as centre. These two steps correspond to the components of motion we are considering.

The step *ST'* is immedia ely apparent to the observer only in this way : that whereas, in 1870, he had to point his telescope in the direction *OS* in order to see the star, in 1920 he must look for it in the direction *OT'*—the directions being measured, of course, with respect to the fundamental standards we have described. The angle *SOT'* is called the *proper motion* of the star, and is part of the data necessary in order to determine the component *ST'*, across the line of sight, of the star's motion.

It is important to notice that the proper motion is, in itself, not a velocity at all, but merely an angle. It corresponds to the apparent change of position of the star in the sky during some stated period of time. Standing by itself, it tells us nothing whatever about the speed or the direction of motion of the star. If the star had moved from S_1 to S_1' or from S_2 to S_2' in the same period, both components of its velocity would clearly have been different but the proper motion would have been exactly the same. To convert the proper motion into the actual component of velocity across the line of sight, we must know the distance of the star. Then, multiplying this distance by the angle *SOT'*, we obtain the length *ST'* travelled, across the line of sight, by the star in fifty years, and therefore the cross-component of its actual speed.

The component *T'S'* is the velocity in the line of sight, or the *radial velocity*, of the star. Unlike the other component, it is measured directly as a velocity. The measurement is possible because of the Doppler principle (p. 34). The direction and magnitude of the radial velocity are determined by the displacement of the spectrum lines which, it is believed, can be measured with a high degree of accuracy. It should be noted that the radial velocity might be represented either by *ST* or by *T'S'*.

There are comparatively few stars—at least, among the brighter ones—whose proper motions are too small to be observed. Time is always on one's side in this measurement. If a star shows no appreciable displacement in twenty years, it

may do so in fifty years, and the longer the interval allowed, the more accurate will be the measurement. Bradley's catalogue of stars—which was compiled about the year 1755 and was revised much later by Auwers—is the basis for a large number of proper motion determinations. We have already said—and the statement will bear repetition—that our ages are but moments in the history of a universe. We may, without sensible error, consider a star's velocity as constant in both magnitude and direction over periods of hundreds of years. We have, of course, to make allowance for the fact that our observations are made from the Earth and not from the Sun. The difference in position is very slight compared with the scale of the Universe, but it is extremely important, as the next chapter will show us. The proper motion is the angle subtended by the star's path at the *Sun*, and not at the Earth.

The largest proper motion known for any star is 10.3 seconds of arc (10″.3) per annum. This motion—which belongs to a star discovered by Barnard in 1916—would change the apparent position of the star by an amount equal to the Moon's angular diameter in about 180 years. If every star had this proper motion, the constellations would have completely changed their appearance within historic times. An average value for proper motions is about 5″ per *century*, so it may be judged how exceptional is Barnard's star. Other stars are known, however, which approach it in this respect. A large proper motion may indicate a large velocity or a small distance. Since the range of distances is much greater than that of velocities, it is nearness that is generally indicated. Catalogues of proper motions therefore contain data for a collection of stars characterised chiefly by nearness but partly also by high velocity.

Like proper motions, the radial velocities of stars cluster rather thinly about a mean value, with some outstanding exceptions. As a general rule, radial velocities, when corrected to the Sun as an observing station, lie below 40 miles per second. Of the very high radial velocities, the star Lalande, 1966, affords the extreme example, with a value of 203 miles per second. The largest known complete velocity of a star in space, on the other hand, is that of the " cluster variable " star, *RZ* Cephei, which moves with a speed of over 625 miles per second. The accuracy of radial velocity determinations is very great. It is claimed that, for stars

whose spectral lines are sharp and distinct, the radial velocities can be determined to within one-sixth of a mile per second.

The limitations of radial velocity measurement are only partly identical with those of proper motion determination. The *sine qua non* for the former operation is that the star shall appear bright enough to give a measurable spectrum. The apparent brightness of a star, as we have seen, depends on its absolute brightness and its distance. Radial velocity catalogues, therefore, like proper motion catalogues, contain data for stars qualified by their nearness, but the selection will be tempered this time by the inclusion of stars of small absolute magnitude (*i.e.* great absolute brightness), instead of those of high velocity. And, since the range of absolute brightness of the stars is much greater than the range of velocity, the selection of stars for radial velocity determination will be relatively much less influenced by distance than will the selection for measurement of proper motion.

We can see now why it has not been practicable to determine the solar motion from the actual space velocities of the stars, after the simple manner indicated on p. 89. To obtain the space velocities, we should have had to convert the proper motions into cross velocities from our knowledge of stellar distances. Until recently, we could determine these distances only for the nearest stars, so that our knowledge of the cross-components—and, therefore, of the whole velocities—would have been limited almost completely to the stars which happen to be in our own corner of the Universe. Modern methods of measuring stellar distances have partly overcome this difficulty, but, until now, the most reliable determinations of the solar motion we have possessed have been made separately from the proper motions and the radial velocities.

The determination of the solar motion from proper motions is only partial. Since proper motions are not velocities, they cannot tell us what is the Sun's speed. They can, however, show us the direction in which it is moving. The principle of the method is as follows. Suppose all the stars are at rest except the Sun, which is moving through the Universe at a certain speed. To an observer on the Sun, the stars will appear to file past him, just as the landscape seems to move in the opposite direction to a traveller in a train. The nearer objects will appear to move more quickly than the distant ones, but that need not concern us in our present inquiry. All the

stars will thus appear to have a sort of false proper motion, really arising from the Sun's motion. A little reflection will show that the arcs along which these proper motions take place must lie in planes through the line of motion of the Sun. Now all planes of this kind must cut the celestial sphere [1] in two opposite points—the ends of the Sun's line of motion. Consequently, the two points, in opposite parts of the sky, towards and from which, respectively, the false proper motions appear to be directed, will define the line along which the Sun is moving.

Another, and perhaps simpler, method of looking at the matter is this. The stars in the part of the sky in front of the Sun will appear to be opening out as the Sun approaches them—in accordance with the ordinary rules of perspective—while the stars behind the Sun will appear to be closing in. The centres of the regions of expansion and congestion are, respectively, equivalent to the points abandoned and sought by the false proper motions to which we have referred. They are called the *apex* and *antapex of the Sun's way*; or, more briefly, the *solar apex* and *solar antapex*.

We have supposed that the stars are all at rest, which certainly is not true. But, from the nature of the standard of rest we have chosen, the method is valid if, in its application, we use all the stars we can deal with. For the false proper motion still exists, though actually it is so blended with a star's true proper motion that, if we consider one star at a time, we cannot separate the two factors. But if we take the observed composite proper motions of all the catalogued stars—or what we are sure is a truly representative selection of them—and compound them all together, then, since we have agreed to regard the mean motion of all the stars as zero, the true proper motions must necessarily cancel out and leave us a balance containing the false proper motions only. The method then proceeds just as if the stars were at rest individually. The assumption is involved, of course, that the standard of rest defined by a mean zero velocity for all the stars is the same as that defined by a mean zero proper motion. The "false proper motion" of which we have spoken—that part of a star's proper motion which arises from the motion of the Sun—is often called the *parallactic motion*, or *secular parallax*.

[1] The *celestial sphere* is the spherical surface on which all the stars appear to lie. The observer is always at its centre.

The determination of the Sun's motion from radial velocities is made in a very similar way, but it gives us the speed as well as the direction of motion. If we assume, as before, that the other stars are all at rest, then the stars directly in front of the Sun will appear to be approaching with the Sun's speed, while those exactly behind will appear to be receding at the same rate. The other stars in the sky will have smaller apparent radial velocities, depending on their directions with respect to that of the Sun's way. In the actual case the stars are not at rest, but their individual motions, when combined together, neutralise one another and leave the conditions the same as if the stars had no motion at all.

The results of the most reliable investigations along these lines are as follows. Lewis Boss, using the proper motion method, finds that the Sun is at present moving towards a point in the constellation Hercules, whose approximate position is given by right ascension, 18 hours; declination, N. 34°.[1] Campbell, on the other hand, from radial velocity measurements, finds that the solar apex is at right ascension, 18 hours; declination, N. 25°; and that the Sun is moving towards this point with a speed of a little more than 12 miles per second. Stromberg, more recently, has made a fresh determination from radial velocities, and finds a right ascension of 18 hours, a declination of N. 29°, and a speed of 13½ miles per second.

The most serious discordance here is in the value of the declination. Granting the substantial accuracy of the measurements of proper motion and radial velocity, there can be only one interpretation of this: namely, that the mean motions of the stars selected for the respective determinations are not the same; or, in other words, the standard of rest defined by each group differs from the standards defined by the others. Why this should be so; how the analysis of stellar motions can be made which will show these differences in the simplest way; and which standard is the closest approximation to the ideal, are questions of the greatest

[1] The *right ascension* and *declination* of a star define its position in the sky, just as longitude and latitude define the position of a place on the Earth. The right ascension corresponds to longitude, and is measured eastward along the celestial equator from a point known as the *First Point of Aries*: it is usually expressed in sidereal time units. The declination is the equivalent of latitude, and is measured in degrees.

difficulty, which we have only just begun to answer. Perrine has suggested and supported the idea that the stars in the northern hemisphere of the sky are, on the whole, moving with the Sun, and that the mixing, in different proportions, of the northern and southern stars in the various determinations of the solar motion is responsible for the discordance. We shall see shortly, however, that a more definite explanation can now be given.

The solar motion has been determined, by a single method, separately for specific groups of stars. Any difference in the results must indicate, of course, that the groups as a whole have mean motions with respect to one another. It appears that there is little, if any, relative motion between complete groups of stars in different portions of the sky—with the possible exception pointed out by Perrine. If we separate the different spectral types, however, we find that the apex determined from the later types is somewhat north of that indicated by the earlier ones, and shows a relative displacement also in right ascension. Again, the larger the apparent magnitude of the stars chosen for the investigation, the more northerly is the apex.

The most striking effect of star selection, however, has been brought out by Stromberg, in a very recent investigation of the solar motion from the *space velocities* of giant and dwarf stars separately. From 800 giants he obtains a solar motion of nearly 12 miles per second, towards the point, right ascension, 18 hours, 12 minutes; declination, N. 37°; while from 415 dwarfs, he finds the astonishingly different value of nearly 20 miles per second towards the point, right ascension, 18 hours, 44 minutes; declination, N. 30°. The explanation seems to be that the dwarf stars have some kind of systematic motion of their own. There is still much to be learnt about this matter, but there can be no doubt that this phenomenon is closely connected with the disagreements between the earlier results, which were derived indiscriminately from giant and dwarf stars.

Another interesting point is the variation of the calculated solar motion according to the velocities of the stars from which it is determined. Stars with velocities less than 40 miles per second give a value agreeing closely with the results of Boss and Campbell. The more quickly moving stars, however, give systematically larger values for the right ascension and declination of the apex, and a much higher figure

for the Sun's speed. It appears that the fastest stars are not distributed in the same way as those of normal speed. They show a very asymmetrical distribution, and are moving, as a whole, towards a point in the Milky Way situated in the constellation Argo. They show a conspicuous disinclination to move towards the part of the sky between the constellations Aquila and Cassiopeia : there is not a single known star, whose speed exceeds 65 miles per second, that is moving in this general direction. The great majority of these very rapid stars are dwarfs of type *F*.

It is evident that the motions of the stars as a whole are not entirely at random. The final analysis has yet to be made, and it is not to be expected that the peculiarities which have come to light in the investigation of the solar motion will all be explained at once. Nevertheless, systematic stellar motions of more than one kind have been well established, and it is our business now to give them brief consideration.

Supposing the solar motion to be known, the measured velocities of the stars can be corrected so as to refer to the conventional standard of rest. Any observed preferential motions of groups of stars will then be real, and will constitute an addition to knowledge. Leaving for a later chapter the discussion of double and multiple stars—which show the simplest and most obvious departures from random motions—the first phenomenon to which we draw attention is that of *moving clusters*. It has been noticed in more than one instance that stars, sometimes in widely separated positions in the sky, seem to have proper motions directed towards a common point—probably indicating that their movements are parallel to one another, since we see them projected on a sphere. This, in itself, might be accidental : in a universe of millions of stars, we should expect to find many groups fulfilling this condition. But if the stars in question have some other quality in common, the evidence for their association becomes very strong. They might, for example, all be of exceptional brightness, or all have an exceptionally large or exceptionally small space velocity. Stars which, from these or other equivalent considerations, appear to have some sort of connection with one another, in addition to that arising from a common direction of motion, are said to form a moving cluster.

A considerable number of moving clusters have already been discovered. There is, for example, one in the constellation Taurus, which includes part of the Hyades group. Most of the bright stars of the Plough take part in another, known as the Ursa Major cluster, which includes Sirius and other stars in equally divergent directions. So far as can be seen at present, the stars in a moving cluster have no appreciable existing bond of union ; their association indicates a common origin rather than a dynamical system. They are not all alike in form. The Taurus cluster appears to be somewhat globular, while Turner has shown that the Ursa Major cluster is remarkably flat. A difference of this kind, however, might have arisen from the different environments of the two groups ; it does not imply that they were originally of different character. Probably a moving cluster is a structure of the same kind as the open clusters (*e.g.*, the cluster M 11 shown in Plate III), seen from a comparatively close standpoint so that the stars appear much more widely separated than they do in the typical open cluster.

Systematic stellar motions of a much more radical kind were brought to light in 1904 by Kapteyn. He showed that, apparently imposed on and independent of the random motions, or *motûs peculiares*, of the stars, there is a tendency for the stars to drift in two opposite directions in the plane of the Milky Way. With the exception of a group which, as a whole, is practically at rest, this phenomenon (known as *star streaming*), appears to affect all the stars in the sky. The number of stars tending to stream in one direction is about half as large again as the number which choose the opposite direction. Kapteyn obtained this result originally from the proper motions of the stars : he and Adams have since confirmed it from a study of radial velocities.

It must not be imagined that all the stars are moving exactly in the directions of their respective streams. As well as the stream motion, they have their individual movements, which may be in any direction. Nevertheless, the general streaming tendency is decidedly marked. The stars in each stream may be compared, with some reservations, to a number of children on a road : the individual children may have widely divergent lines of movement, but the general tendency is towards the school. The points in the sky to which the streams are tending are called the *vertices*. The

vertex of stream I.—the more populous of the two—is at right ascension, 6 hours, 20 minutes; declination, N. 12°: that of stream II. is at the opposite point—right ascension, 18 hours, 20 minutes; declination S. 12°. It should be mentioned, however, that the position of the principal vertex varies somewhat with the average velocity of the stars chosen for its determination, though remaining always in the Milky Way. This would seem to indicate that the individual motions of the stars, apart from the effects of star-streaming, are not quite at random, and that there are yet other systematic motions to be discovered. It has been claimed, in fact, that the phenomena are best represented by the assumption of three drifts instead of two, and yet other ways of looking at the matter have been proposed. We shall, however, adhere to the two drift hypothesis as the one most in favour. The relative velocity of the two streams is about 25 miles per second. The streams seem to be fairly well mixed in all parts of the sky—like two swarms of insects moving in opposite directions through the same region of space.

Explanations of star-streaming have been proposed, but no definite conclusion has yet been arrived at. Turner has suggested that the majority of the stars are moving in very elongated orbits about a centre which he places in the direction of the principal vertex. The components of motion towards and from the centre would then greatly predominate over the transverse components, and would give the appearance of two opposite streams. We shall meet with other explanations in a later chapter. It is evident that star-streaming is a phenomenon of fundamental importance in the operations of the Universe, and no theory of cosmogony can be accepted which fails to give a suitable account of it.

Turning now to the relation between the movements of stars and the spectral types, let us recall the different kinds of movement which we must take into consideration. There is, first of all, what we may call the individual motion of a star, which we may regard as a characteristic of the star itself, and not part of a group movement. Then, coming to the group movements, there is, first, the apparent movement arising from the motion of the Sun, the cross component of which we have called the parallactic motion. We have already referred to the connection of this phenomenon with spectral type, in saying that the position of the solar apex varies with the

types of the stars chosen for its determination : we will not at present pursue it further. Next, there are the groups of stars which form moving clusters. And, finally, the relation of star-streaming to spectral type calls for attention.

We should not, from *a priori* considerations, look with very great confidence for a relation between velocity and spectral type. The velocity of a star has no absolute value : we can make it whatever we please by a suitable choice of a standard of rest. The spectral type, on the other hand, as we have every reason to believe, represents one of the most complete confessions of its character that a star can make. It is a matter for surprise, then, when we find that the individual velocities of the stars, irrespective of direction, increase steadily with advance of spectral type. There is no doubt about the fact : it has been derived independently from the radial velocities and the proper motions, corrected for parallactic motion, and from the complete space velocities themselves. It does not matter whether or not we eliminate from the velocities the part due to star-streaming : the effect is shown in either case, though, of course, star-streaming must be allowed for before we conclude that the actual individual (the so-called " random ") motions of the stars are related to spectral type.

In view of the division of stars of all—or, at least, the later—spectral types into two classes, giants and dwarfs, which came to light in a discussion of absolute magnitudes, we must inquire how this relation appears when applied to giants and dwarfs separately. This matter has very recently been investigated by Adams, Stromberg, and Joy. They find that, for both giants and dwarfs, there is a progressive increase of average space velocity with advance in spectral type. Considering the types *F*, *G*, *K*, *M*, where the giant and dwarf division is clearest, they show that, among the giants, there is a regular increase of mean space velocity from about 13½ miles per second for type *F* to about 18 miles per second for type *M*. The departures of individual velocities from the mean value in each type are not very large. Among the dwarfs, on the other hand, the individual velocities are considerably greater, and show large variations from the mean values. The progression with spectral type is not so definite as it is for the giants. There is a progression, however. The average velocity for type *F* is 23 miles per second, while that for type *M* is as high as 34 miles per second.

Nearly the whole of the increase occurs between types *F* and *G*.

The results seem to indicate that the giants form a much more homogeneous class of stars than do the dwarfs. In types *B* and *A* it is not easy to separate the giants from the dwarfs. Taking both classes together, Campbell finds mean *radial* velocities of 4 and 7 miles per second for types *B* and *A* respectively. The space velocities will be, on the average, about twice as large, so that we may take them as 8 and 14 miles per second. The figures are therefore quite consistent with a continuous increase of velocity throughout the sequence for each of the classes, giants and dwarfs.[1] These facts are extremely interesting, particularly in view of their unexpectedness. In a later chapter we shall find some light thrown on them from a consideration of other stellar qualities.

In turning to the systematic motions of the stars, we should note, first of all, that Campbell, in his radial velocity measurements, finds a curious effect,[2] which is greatest for the stars of type *B*. It appears that, if the measures are to be accepted as they stand, the *B* stars are systematically receding from the solar system : they appear to form a group which is expanding outwards at the rate of about 3 miles per second. This is a remarkable conclusion, which can hardly be accepted at its surface value. We hesitate to believe that we occupy the central position of a special group of stars, and, indeed, the evidence is against the notion. There are a number of possible explanations of the result, but none has yet been definitely established. The possibility of displacement of spectral lines from causes other than the Doppler effect must be borne in mind. It appears that the relativity displacement (p. 40), is not sufficient to account for the whole of the phenomenon, but there may still be displacements arising from sources as yet unknown. Albrecht, for example, claims to have found evidence for a progressive shift of certain spectral lines with change of spectral type. The whole question of displacement of spectral lines calls for further study.

[1] It should be noted that Adams and Joy, in a study of stars of exceptionally high velocity, found that two-thirds of the stars they dealt with were of type *F* or *G*—a remarkable result, which might possibly be explained by considering the masses of the stars (see Chap. XI.).

[2] Campbell's "*K*-term."

The *B* stars have another peculiarity, of the validity of which there can be no doubt. They have a strong tendency to take part in moving clusters. A considerable proportion of them are included in these groups. Moving clusters, as a rule, show a preference for stars of early type, but some later type stars are to be found in them. There is a general, but not very rigid, uniformity in the spectral types of the stars in any one cluster—which would, perhaps, be expected, on the assumption that the cluster association denotes a common origin.

In considering the stream motions of stars in relation to spectral type, we again come across a peculiarity of the *B*-type stars. They have very small stream motion and, until recently, were thought to have none at all, and to form, in fact, a large majority of the group of stars ("stream *O*"), which we have mentioned as being at rest as a whole. Kapteyn, however, in a special study of these stars, showed that their motions were partly due to streaming: nearly the whole of them belong to stream I. In the stars of type *A*, on the other hand, streaming is seen in its most pronounced form.[1] It is to be found in the later types also, but gradually decreases in prominence with advancing type. This does not necessarily mean that the stream motion is dying out. The individual velocities increase as the spectral type advances, and the appearance of streaming consequently tends to be overwhelmed by these more random motions. We have to suppose that, for some reason, *B*-type stars are almost exempt from streaming, while the remaining stars probably share in the relative drift of 25 miles per second, irrespective of type. It seems that, on the whole, stream I. contains a larger proportion of late type stars than does stream II.

[1] Since this chapter was written, Stromberg has announced that, from a study of the space velocities of 332 stars, it seems clear that, in type *A*, Kapteyn's first stream may be identified with the Taurus moving cluster, and the second stream with the Ursa Major cluster. The remaining *A* stars which he has studied appear to belong to stream *O*, the system practically without motion. The results for types *F* to *M* show that the giants form a single group with small systematic motion. The streaming tendency diminishes from *F* to *M*, where it practically dies out. The dwarf stars also show stream motion, but the directions of the streams are entirely different from those of the giant stars. Particular attention is directed to the fact, which we have already pointed out, that rapidly moving stars, regardless of spectral type, show a complete avoidance of motion in the direction of the Galaxy between the constellations Aquila and Cassiopeia.

The whole question of stellar movements is of extreme interest and importance. When we consider how inconceivably small are the apparent changes of position from which they are in large part detected—the constellations have undergone no appreciable change of appearance within historic times—we cannot but be impressed with the magnitude of the results achieved. They offer, indeed, one more example of the truth that no fact is too trivial, if carefully observed and brought into relation with other facts, to open a fresh door into the mysteries that surround us.

CHAPTER VIII

STELLAR DISTANCES

In the preceding chapters we have more than once come across characteristics of stars, for a knowledge of which we are dependent on measurements of stellar distances. It is time now that we saw how those distances can be found, and, consequently, how far we are limited in our knowledge of the characteristics in question. The distance of a star—though of little importance in itself, for it is determined partly by our own arbitrary position in space—is, nevertheless, one of the quantities which we can least afford to neglect. Not only is it a necessary factor in the investigation of more intimate stellar qualities: it is indispensable also to a full knowledge of the form and scale of the Universe. We turn, then, to a brief review of the methods by which the distances of the stars are measured.

The fundamental method depends on the Earth's orbital motion round the Sun. In the last chapter we defined the proper motion of a star as its apparent change of position when viewed from the Sun. It is clear that, if the observation is made from the Earth—as in practice, of course, it always is—there will be an apparent periodic change of position of the star superposed on the true proper motion, owing to the Earth's annual revolution round the Sun. Thus, for example, in Fig. 10 (in which the size of the Earth's orbit is greatly exaggerated compared with the distance of the star), if a star S has no proper motion, it will appear, when seen from the Earth, to describe a small ellipse in the sky. When the Earth is at E_1, the star will appear in the direction E_1S': when the Earth is at E_2, it will appear in the different direction, E_2S'': and so on; a complete revolution of the Earth corresponding to the apparent description of a small ellipse in the sky. Now the length of this ellipse will depend only on the distance of the star from the solar system: it is measured, obviously, by the size of the angle E_1SE_2. For a star nearer the Earth, such as S_1, the corresponding angle, $E_1S_1E_2$ is larger, and for

a more distant star it is smaller. The size of this angle therefore affords a measure of a star's distance. Half the angle—namely, the angle subtended by the mean radius of the Earth's orbit at the distance of the star—is called the *parallax* of the star, and in practice is generally used in preference to the actual distance.

The principle is applicable, no matter whether the star has a proper motion or not. If it has, the two movements have to be separated, but there is no difficulty in this, since one is periodic and the other is always in the same direction. The annual proper motion is usually much larger in amount than the periodic displacement. The latter is determined in practice by comparing the apparent positions of the star,

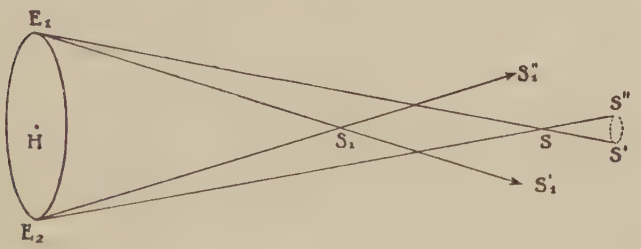


Fig. 10.

at different times during the year, with reference to neighbouring stars whose distances are so great as to make their own parallaxes extremely small. It is, in fact, the chief limitation of this direct method of measuring the distances of stars, that the stars are so far away as to make a measurable parallax a *rara avis*. The nearest star, so far as we know, is a distant companion of α Centauri, which has been called Proxima Centauri. Its parallax is 0″.79, *i.e.*, just over ¾ of a second of arc. The difficulty of measuring so small a quantity as this may be realised when it is pointed out that one second of arc is the angle subtended by a threepenny-piece at a distance of about two miles. Nevertheless, small as stellar parallaxes are, an enormous amount of care has been spent on their determination, and it is now possible to obtain a fairly reliable measure of any parallax which is not less than 0″.03. Stars are generally selected for parallax measurement if they have large proper motions. As we have pointed out, the value of the proper motion is a fair indication of distance,

and the chances are that a star whose proper motion is large will have a measurable parallax.

It will be noticed that the employment of parallax as a measure of distance is similar to the representation of brightness by means of magnitude: the larger the parallax the smaller the distance of a star. There is not the same objection, however, in this case. The parallax is the quantity that is actually measured: the word creates, moreover, no prejudice in the mind of the reader, such as is likely to be aroused by a familiar word like "magnitude." Again, even if we speak in terms of distances, it is necessary to adopt a new unit, so that the figures dealt with shall not be inconveniently large. The unit of distance that is coming into most general use is known as the *parsec*: a star at a distance of one parsec would have a parallax of one second—hence the name. No star is known within a distance of one parsec from the Sun. The great convenience of this unit is that the distance in parsecs is the reciprocal of the parallax in seconds, so that the transition from one to the other can be made mentally. A simple calculation shows that one parsec is equal to about 19 million million miles. The *light year* is a unit of distance that is still frequently used. It is the distance which light, travelling at the rate of 186,000 miles per second, would travel in one year: it is equal to 0.31 parsec.

The small range of application of the direct method—the "trigonometrical" method, as it is called—of measuring parallax has made it necessary to look for indirect means of adding to our knowledge. These can sometimes be found in special circumstances associated with particular stars. If, for example, a number of stars are known to belong to a moving cluster and the parallax of one of them can be measured, we can obtain the parallaxes of all. For the parallax which is known enables the proper motion of the corresponding star to be converted into cross-velocity, and, combining this with the radial velocity, we can get the actual space-velocity and direction of motion of the star. Now it is characteristic of a moving cluster that all the stars in it have the same speed and direction of motion. Hence, from the common space velocity, we can calculate the cross-component of the velocity of each star in the cluster, and, from the observed proper motion, obtain at once the distance or parallax. A considerable number of parallaxes, which are

inaccessible to the trigonometrical method, have been determined in this way.

Or, again, if the two components of a double star can be seen, and revolve about one another quickly enough for their continuous variation of velocity to show itself as an oscillating Doppler effect in the spectrum of the star, sufficient data can be obtained for the dimensions of the orbit to be calculated, according to dynamical principles. The apparent separation of the components depends on the actual linear distance between them and the distance of the double system from the Earth. For stars fulfilling the above conditions both the linear and the apparent separation can be found, so that the parallax of the star becomes known. There are not many stars, however, to which this method can be applied.

By far the most fruitful source of parallax determinations is one which has been developed within the last few years. It will be perceived that the indirect methods consist of the independent determination of a pair of quantities—*e.g.*, proper motion and cross-velocity ; angular and linear dimensions of orbit—whose ratio is the distance, or is a measure of the distance, of the star. Now, there is another pair of quantities fulfilling this condition that will at once spring to mind ; namely, absolute and apparent brightness. We have seen how the absolute magnitude of a star can be determined directly from the spectrum, and the apparent magnitude by photometry. Any star, therefore, whose absolute magnitude can be determined from the spectrum, can be made to provide all the data necessary for the calculation of the parallax. The equation from which the calculation is made is given on p. 80.

It is not too much to say that the discovery of this method of parallax measurement constitutes one of the greatest advances in Astronomy in modern times. The first reliable trigonometrical parallax was determined in 1838, and up to the end of 1921, at the expense of an almost incredible amount of labour, about 1400 measures by the direct method had been made. The spectroscopic method, in five years, has determined 2000 parallaxes, and is daily providing fresh data to be added to the list. It is expected that, within a few years, the spectroscopic method will measure the distances of nearly all the naked eye stars with spectral types from *A*5 onwards. The great advantage of the method is that the distance of the star does not matter, except in so far as it may

diminish the amount of light we receive. With the new 100-inch reflecting telescope at Mount Wilson, the absolute magnitudes and parallaxes of stars of the 9th apparent magnitude have been measured, and the method can be extended to much fainter stars. The most difficult, and, in many respects, the most necessary, of all astronomical measurements can, at last, be undertaken on the large scale.

In celebrating the triumph of the new method, it is necessary to point out that it does not dispense with the trigonometrical determinations. Indeed, they are essential for its application: it could not exist without them. It is the absence of sufficient trigonometrical parallaxes that makes the spectroscopic method difficult of application to the distant *B* stars.[1] The numerical relation between absolute magnitude and the relative intensity of spectrum lines is at present purely empirical: we cannot calculate it from first principles. In order to obtain the curves from which absolute magnitudes are determined by the spectroscopic method, it is necessary to know a considerable number of absolute magnitudes determined by other processes. No other process is known except that which calculates absolute magnitude from parallax and apparent magnitude, and for the parallax we are thrown back, in the main, on the trigonometrical method. It is just like the affairs of everyday life. Capital is necessary in order to start a wealth-producing organisation. Once established, the organisation may produce far more capital than the amount with which it started, but without the original outlay it could never have existed.

It may be remarked, incidentally, that the parallax of a dwarf star can be obtained at once, with a fair degree of accuracy, merely from a knowledge of its spectral type. It will be seen, from Fig. 8, that the range of absolute magnitude of dwarf stars of a given type is not very large. Taking an average value, an approximate figure for the parallax can be determined.

There are a large number of visual binary stars whose

[1] Edwards, at the Norman Lockyer Observatory, Sidmouth, has recently measured the parallaxes of *B* stars spectroscopically, using "hypothetical" (see p. 109) and other parallaxes to obtain the curve. Simultaneously, new trigonometrical parallaxes of *B* stars have been issued by the M'Cormick Observatory.

orbits are known with considerable accuracy. We have already pointed out that if, in addition, the changes of velocity of their components can be detected with the spectroscope, the parallaxes can be calculated. If, as is usually the case, this is not possible, we can obtain only a relation between the total mass of the system and its parallax, both of which are unknown quantities. It is somewhat annoying to have a large collection of ambiguous data of this kind. Fortunately the knowledge of stellar masses that we have (see Chap. X.) assures us that, neglecting a very few exceptional stars, the masses are not many times greater or less than that of the Sun: there is a rough uniformity in the distribution of mass among the stars. If, then, we assume that the two components of a double star, taken together, have twice the mass of the Sun, we can calculate the parallax.

A parallax calculated in this way is called a *hypothetical* parallax. It may, of course, for any individual star, be considerably in error, but the general results for a large number of stars will give a pretty accurate idea of their mean distance. It is fortunate that any error in the assumed mass leads to a much smaller relative error in the parallax. The formula is

$$\text{Parallax } (\pi) = \frac{a}{[\mu P^2]^{\frac{1}{3}}},$$

where a = semi major axis of orbit, in seconds of arc; μ = sum of masses, taking the mass of the Sun as the unit; and P = period of revolution, in years. Since the sum of the masses occurs to the power $\frac{1}{3}$, the hypothetical parallax is only twice the real value if the masses are really eight times as great as the estimate.

Jackson and Furner have determined a large number of hypothetical parallaxes in this way, and they have extended the method to double stars whose complete orbits are not known. The quantity $\frac{a}{P^{\frac{2}{3}}}$ on the right hand side of the above equation can be determined with considerable accuracy when only a very small part of the orbit has been observed, so that our knowledge of hypothetical parallaxes is not restricted to those stars for which the individual quantities a, μ, and P can be found or assumed. So long as even the slightest relative motion of the components is discernible, the hypothetical parallax can be calculated.

Stellar parallaxes play so important a part in the wider problems of Astrophysics that no possible information about them is too trivial to be sought after. When all known methods of measuring individual parallaxes—actual or hypothetical—are exhausted, it is still possible to obtain mean parallaxes for particular groups of stars. There are two possible methods of doing this, both depending on the apparent motions of the stars.

We have seen how the observed change of position of a star in the sky, when viewed from the Sun, is made up of two parts—the true proper motion and the parallactic motion arising from the motion of the Sun itself. We cannot separate these two factors for any one star unless we know the parallax of the star, because, obviously, the nearer the star is to us, the greater will be the parallactic displacement. We can, however, partially separate it from the true proper motion in the following way. Whatever the parallactic motion may be, we know that it must take place along the great circle in the sky passing through the solar apex and antapex and the star, and, further, that the motion must be directed along this circle from the apex towards the antapex. Suppose, then, the apparent changes of position of a large number of stars to be resolved, each into two components—one along the great circle through the apex and antapex, and the other at right angles to it. The whole of the parallactic motion must then be contained in the first component, while the second is determined only by the proper motion of the star. The two measurements are known, respectively, as the υ- and τ-components, and each gives rise to a method of determining a mean parallax for a group of stars.

Consider first the υ-component. If a star had no proper motion, this component would consist entirely of the parallactic motion. Its magnitude would depend only on the distance of the star, the direction in which it is seen with respect to the direction of the solar motion, and the magnitude of the solar motion itself. The last two of these quantities are known, so that the observed value of the υ-component would give at once the distance of the star. Since the stars have proper motions, the method cannot be applied to each one individually. Taking a number, however, the individual motions cancel out (cf. p. 94), and we are left with an average consisting only of parallactic motion. The average distance

calculated is therefore the average distance of the group of stars.[1]

The determination of mean parallax from the τ-component is similar, but needs additional information to take the place of the parallactic motion used in the first process. This is found in the radial velocities. It can easily be shown that, for a number of stars moving at random, the average component of velocity along any line is half of the average total velocity. Thus, the average radial velocity and the average τ-component will each be half the average space velocity of the stars, when allowance has been made for systematic motions. (It is necessary, for this to hold, to take the numerical values of the velocities and their components, without regard to direction. If the direction were taken into account, the average would be zero, owing to our definition of a standard of rest.) Now we can measure the radial velocities and find their average value, which will be also the average value of the velocities along the τ-direction. Further, we can measure the τ-components as proper motions, *i.e.* in angular measure, and get their average value in these terms. The ratio of these two quantities gives at once the average parallax of the group of stars.[2] The method can be applied only to the determination of *average* parallaxes, because it is only when a large number of stars are taken that we can equate the components of velocity along the line of sight and in the τ-direction. To obtain accurate results it is necessary to choose stars which have not a very wide range of distance.[3]

In previous chapters we have come across two data of

[1] The formula used is: Average parallax $(\bar{\pi}) = -\frac{\overline{\upsilon \sin \lambda}}{V_0 \overline{\sin^2 \lambda}}$, where V_0 = speed of solar motion; λ = angle between direction of star from Sun and direction of Sun's motion; and the value of υ is taken irrespective of direction. A bar over the quantities indicates that the average value is to be taken.

[2] The formula is: $\bar{\pi} = \frac{\bar{\tau}}{\bar{V}}$, where $\bar{V}$ is the average value of the velocity components along the τ-direction.

[3] Under certain stated conditions, Russell has shown that, for stars whose velocities are less than 9 miles per second, the υ-component gives the better result, while, for the faster stars, it is better to use the τ-component.

observation, each of which involves the distance and another property of a star. They are, respectively, the apparent magnitude and the proper motion. The former involves distance and absolute magnitude; the latter, distance and velocity. If all the stars had the same absolute magnitude and the same space velocity, the apparent magnitude or proper motion would, obviously, tell us at once the relative distance of a star. The stars, however, have various absolute magnitudes and space velocities, but if we knew exactly how they were distributed in space with respect to these properties, we should still be able to calculate distances from apparent magnitudes or proper motions, though the calculation would not be so simple. Conversely, if we knew the distances of all the stars whose apparent magnitudes and proper motions we observe, we should be able to calculate how the absolute magnitudes and space velocities are distributed in the Universe.

The last-named problems have received a great deal of attention from astrophysicists. In accordance with what we have just said, the preliminary step has been to express in a mathematical form the distances of stars in terms of their magnitudes and proper motions. The expression must be a very general one, and can hold only in the average, for, obviously, the distance of a star is only partly revealed by these two quantities. Kapteyn has calculated two mathematical expressions—the first representing the average parallax of stars of a given apparent magnitude, and the second representing the average parallax of stars of a given apparent magnitude and given proper motion. From the measurements of parallax carried out by the methods we have described, he has given his expressions numerical form and found them satisfactory.[1]

Strictly speaking, Kapteyn's formulæ do not add to our knowledge: they simply express it. They do not provide us with a new method of measuring parallax. We give them here because it seems fitting, after an account of the direct and indirect processes that have been applied to the determination of stellar distances, to show how the results have been

[1] Kapteyn's formulæ are: $\overline{\pi} = A\epsilon^m$; $\overline{\pi} = A_1\mu^b\epsilon_1{}^m$; where $\overline{\pi}$ = mean parallax, μ = proper motion, m = apparent magnitude, and A, A_1, b, ϵ, ϵ_1 are constants. The latest numerical form given by Kapteyn and Van Rhijn to the second formula, after a slight transformation, is $\log_{10}\overline{\pi} = -0.691 - 0.0682m + 0.645 \log_{10}\mu$. Adams and Stromberg have claimed that better representation of the facts is given by adding another constant, c, to the proper motion, μ, thus: $\overline{\pi} = A_1(\mu + c)^b \epsilon_1{}^m$.

summarised for the larger aims of Astrophysics. We shall show later how the formulæ are used in attacking the fundamental problems of the structure of the Universe.

In seeking for a relation between distance and spectral type, we are confronted by the difficulty of eliminating other factors that intrude on the problem. Thus, if we use trigonometrical parallaxes, we are confining ourselves to the nearest stars. If we use spectroscopic parallaxes, we give preference to the brightest, and, since brightness is very definitely connected with spectral type, we might find a relation which does not depend on the last-named property at all. Investigators, as a rule, have preferred to take groups of stars having the same apparent magnitude, and to calculate the mean parallaxes from the parallactic motions in the way we have described. Here again, however, the effect of intrinsic brightness creeps in, particularly in the failure of the method to take account of the difference between giant and dwarf stars.

However, taking the results as they stand, it appears that, as we progress from type *B*, the mean parallax increases (*i.e.* the stars are, on the average, nearer to us) until type *F* is reached, and then decreases again to type *M*. The most distant stars, however, appear to be those of type *B*.

The whole problem of stellar distances has taken on a new aspect since the development of the spectroscopic method of measurement, and it is hardly possible yet to estimate the scope of the deductions which will be possible as the data accumulate and their full significance is realised. The bearing of the facts on the general questions of cosmology depends on the position we occupy in the Universe and the relative depth to which our telescopes allow us to pierce. We are not without ideas on these matters, but it would be exceedingly rash to suppose that we have yet approached finality. Meanwhile, the results of parallax determinations and their relation to the rest of our knowledge play a leading part in the drama which is gradually unfolding itself.

CHAPTER IX

STELLAR TEMPERATURES AND COLOURS

"ARE not the Sun and fixed stars great earths vehemently hot?" asked Newton, not knowing that he was himself laying the foundations of the process which was to answer his question in the affirmative. We return in this chapter, after an apparent digression into the seemingly arbitrary properties, velocity and distance, to intrinsic qualities of the stars; namely, their temperatures and colours.

There is a close connection between these two qualities, which is almost one of cause and effect. The temperature of a body determines the relative amounts of different kinds of radiation that the body will emit, and on these relative amounts depends the colour. We cannot, however, assume that the connection is so simple as this, because of the intrusion of another factor. The light of the majority of stars forms continuous spectra, and on these are superposed absorption lines and bands. The presence of a powerful band in any region of a spectrum means the absence, in great part, of the light corresponding to that region, and since the character and position of the band depend partly on the chemical composition of the star's atmosphere, it is not to be assumed that, in measuring temperature and colour, we are measuring two aspects of the same thing. The temperature determines the distribution of radiation in the original continuous spectrum: the colour is a statement of the distribution of radiation in that part of the original continuous spectrum which the star's atmosphere permits us to receive. It is necessary, therefore, to determine temperature and colour by different methods.

According to the ordinary meaning of the word, *temperature* must be associated with a material body. It is possible, however, to generalise the conception, measuring the temperature at a point in "empty space" by the temperature which some standard body would ultimately reach if it were placed at that point. If we knew the distribution of this

generalised temperature throughout the Universe, we should, no doubt, find the knowledge of enormous assistance in solving the problems of Astrophysics. There seems, however, to be no means of knowing it until those problems are actually solved. From our present point of view, therefore, we shall not attempt to discuss temperature in this wider sense : we shall confine ourselves to the temperatures of the stars.

And, even in dealing with a single star, we must state more precisely what we mean. It is not likely that the temperature of the surface, from which loss of heat by radiation is continually taking place, will be so high as that of the blanketed interior : we should expect, in fact, that, from the surface inwards, a star would be hotter and hotter. A complete statement of the temperature of a star would be a mathematical expression, showing not only the temperature at one point, but how the temperature varies from point to point in the star. In the present state of science, it is not possible to determine this experimentally. As usual, we have to be satisfied with something less than we should wish for.

We have already described (p. 28) a method of measuring the temperature of a body which radiates a continuous spectrum, from considerations of the distribution of energy in the spectrum. If we take the continuous part of a stellar spectrum, find the length of the waves that convey the maximum amount of energy, and divide that length into a constant quantity, we obtain a certain temperature, which is equal to the temperature to which a perfect radiator must be raised in order to give a similar spectrum. This temperature is called the " effective temperature " of the star.

The question arises : what part of a star has a temperature equal to the effective temperature ? If we can assume that a star is actually a perfect radiator, we can say at once that it is the portion that radiates the continuous spectrum. But now, what portion is that ? It is certainly not the outer atmosphere, for that gives us the absorption lines. All we can say with certainty is that it is some part of the star beneath the absorbing atmosphere, which may be a thin layer or may be a region of considerable depth, whose *resultant* outward radiation produces the continuous spectrum we see. For the present, we will consider the effective

temperatures of stars without inquiring too closely into their precise meaning.

A considerable number of effective stellar temperatures have been determined by Nordmann at Paris, and by Wilsing and Scheiner at Potsdam.[1] Wilsing and Scheiner compared the distribution of energy in the spectrum of a star, between the wave-lengths, 4480 and 6380, with that in the spectrum of an electric glow lamp. Measurements were taken of the relative amounts of energy at five different points within this range, the two sources being reduced to apparent equality of brightness by means of Nicol prisms. Since the important thing is the distribution of energy in the spectrum, and not the actual amount of energy at any point, no error was introduced by thus reducing the intensity of either of the spectra. The spectrum of the glow lamp was then compared with that of an oven maintained at an absolute temperature of 1500° and arranged to give the equivalent of perfect radiation. Not only was the wave-length of maximum energy observed, but the distribution of energy throughout the whole region considered was obtained for each spectrum. The effective temperature of the star was then calculated by a well-known formula given by Planck.[2]

The correlation of effective temperature with spectral type is so strong that the results of these experiments are always stated in terms of spectral type. The following table shows the values given by Wilsing in a recent revision and amplification of the earlier work of himself and Scheiner. The results are based on studies of 199 stars.[3]

[1] A useful summary and discussion of a number of determinations of stellar temperatures will be found in a lecture by E. A. Milne on "Recent Work in Stellar Physics." (*Proceedings of the Physical Society of London*, Vol. 36, p. 94, Feb., 1924).

[2] Planck's formula is: Energy emitted per unit range of wave-length at wave length λ, $= c_1\lambda^{-5}(e^{\frac{c_2}{\lambda T}} - 1)^{-1}$, where T is the absolute temperature of the radiator, c_1 and c_2 are constants (c_2 = Planck's "constant of action"), and e is the base of the Napierian system of logarithms.

[3] Sampson has just shown that the accuracy of measurement of the energy distribution in a spectrum can be greatly increased by the use of a photoelectric cell (p. 79) as detector. He has applied such a cell to the measurement of the temperatures of a few stars, and his

TABLE II

Spectral Type.	Temperature (in deg. C.).
*B*0	10500
*B*5	10000
*A*0	9230
*A*5	8110
*F*0	7000
*F*5	6080
*G*0	5300
*G*5	4610
*K*0	3860
*K*5	3270
Ma	3080
Mb	2960
Mc	2890

A connection of this kind is really instructive, for it assures us that there is a considerable amount of continuity between the interior and the atmosphere of a star. The spectral type, determined from the absorption lines, is a classification of some property or properties of the stellar atmosphere: the effective temperature measures a property of the interior source of continuous spectrum. The discontinuity revealed

results indicate that the measures of Wilsing are considerably too low. Assuming the effective temperature of Capella to be 5500°, he has given the following comparative table :—

Temperature.	Star.	Spectral Type.	Absolute Magnitude	Wilsing's Result.
16900	γ Cassiopeiæ	*Bop*		6800
15500	α Andromedæ	*Aop*	1.1	9400
15000	α Pegasi	*A*0		10500
12500	α Cygni	*A*2*p*		9400
11200	α Lyræ	*A*0	0.6	9400
10000	α Aquilæ	*A*5	1.7	8100
9200	α Cephei	*A*5	1.3	6900
8800	β Cassiopeiæ	*F*5	1.6	5100
7500	α Persei	*F*5	−1.3	5700
6800	α Ursæ Minoris	*F*8	−3.0	5600
6600	γ Cygni	*F*8*p*	−3.0	6300
5500	*Capella*	*Go*	*0.1*	*7100*
5100	ε Cygni	*K*0	0.8	4400
4700	α Cassiopeiæ	*K*0	−1.0	
4200	ε Pegasi	*K*0	−0.3	4500
3600	β Pegasi	*M*3	0.0	2800
3500	β Andromedæ	*M*0	0.5	3200

by the change from a continuous emission to an absorption spectrum is evidently far from being an arbitrary one.

When these temperatures were measured, no discrimination was made between giant and dwarf stars. Now that the importance of the distinction between these two classes is more completely realised, we naturally inquire whether there is any difference in temperature between the giants and the dwarfs of the same spectral type. It is to be noted that the stars selected by Wilsing were nearly all giants. This is not surprising, since the stars which are intrinsically the brightest will be, on the average, the brightest to the eye, and will be chosen for examination in preference to the fainter ones. Seares, however, has lately investigated the temperatures of dwarf stars from data obtained in the study of their colours, and he finds that the temperatures are consistently higher than those of the giants, the difference becoming more pronounced with advancing spectral type. Fig. 11 shows the temperatures for the giants of different types in graphical form, while from *F*5 to *Ma*, where the distinction is clear, the dotted graph shows Seares's values for dwarfs. It will be noticed that the temperature decreases continuously from *B*o to *Ma*, whether the giant or the dwarf branch is followed.

In turning our attention to the colours of the stars, we may recall that colour differences distinguish the different spectral types in Secchi's classification, so we shall be prepared for some systematic variation of colour with spectral type. It will be realised at once, however, that unless some more precise meaning is given to the term colour than that which it usually bears, it is not a property which can be subjected to exact measurement. Numerical estimates of colour have, nevertheless, been assigned to stars in the past, but nowadays the general practice is to select some property of a star that is more or less closely related to its colour but is better adapted to precise classification. A number of properties of this kind have been proposed, all more or less satisfactory. We shall confine ourselves to the three in most common use: namely, the *colour index*, the *effective wave-length*, and the *exposure-ratio* of a star.

[1] It has just been announced that Abbot has devised an improved type of vacuum "bolometer," by which it is hoped to investigate the energy distribution in stars down to the 3rd or 4th apparent magnitude. This should add considerably to our knowledge of effective stellar temperatures.

The colour index is defined as the difference, photographic minus visual (or photovisual) magnitude. A star deficient in violet light will have a high photographic magnitude and a much lower photovisual one, so that such a star will have a high colour index. A blue star, on the other hand, which is relatively deficient in red light, will have a low—often negative —colour index. The value of the colour index is thus a

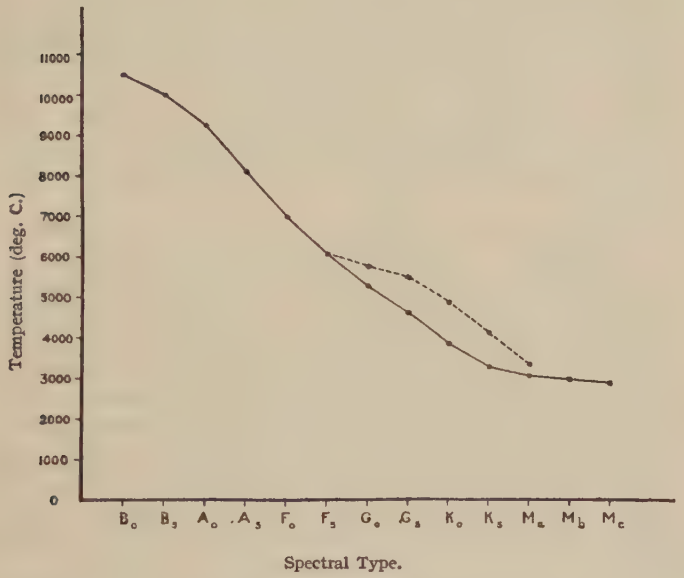


Fig. 11.

Temperatures of stars of various Spectral Types.
(Continuous graph = giants. Dotted graph = dwarfs.)

direct numerical indication of colour. It should be noticed that, owing to the way in which magnitudes are defined, a difference of magnitudes corresponds to a function of a *ratio* of brightness.

The effective wave-length of a star is closely allied, in some respects, to the wave-length of maximum energy, from which the effective temperature is determined. But whereas, in measuring the latter quantity, the absorption lines and bands are ignored, and the curve of distribution of energy is drawn from the bright portions as if the absorption did not

exist—the effective wave-length, on the other hand, denotes actually the region of the composite spectrum at which the light is most intense. It is determined by means of the coarse wire grating already described (p. 79). In Fig. 7 the images on either side of the central one are really spectra with very small dispersion, and the densest portion on any of them will be the region of intensest light as estimated by the photographic plate used. From the dimensions of the grating it is possible to calculate the wave-length at any point in any of the spectra by simply measuring the distance of that point from the central image; the greater the distance, the greater the wave-length, since the violet end of the spectrum will be the nearer to the centre. The wave-length corresponding to the position of greatest intensity in each spectrum is the "effective wave-length" of the star.

Of course, numerous corrections have to be applied to the observations. The general intensity of the images, for example, must be made equal to a certain standard by adjusting the time of exposure: also, the stars must be observed at a fixed distance above the horizon—or the results must be corrected if some other distance is chosen—on account of the selective transmission of radiation by the Earth's atmosphere. The influence of conditions such as these makes it necessary to state precisely how the measurements were made, and the standard conditions are chosen so that the colours corresponding to the accepted effective wave-lengths shall agree with those indicated by the colour indices.

The exposure-ratio is the ratio of the times of exposure necessary to produce photographic and photovisual images of the same size. The dependence of the size of a stellar image on the intensity of light in it was pointed out on p. 78. The smaller the intensity of light, the longer will be the time required to produce an image of a given size. The exposure ratio therefore measures the relative amounts of blue and yellow light in the star, and the scale is adjusted to coincide, as nearly as possible, with that of colour indices.

Each of these three colour criteria has its own advantages and defects. A considerable amount of investigation and ingenuity is needed to adjust the conditions of experiment and the definitions of the respective scales of colour, so that the results of observers with different instruments, in different

parts of the world, shall agree with one another. The measurements have already been established on a fairly sound basis, and the numerical value of a star's colour gives definite information about an intrinsic quality of the star. It should be noted that colour is expressed in magnitudes, following the lead given by the meaning of colour index.

The relation between colour and spectral type is a very intimate one. The following table, taken from colour index measures made at Mount Wilson, shows this clearly :—

TABLE III

	Colour Index.	
Spectral Type.	Giants.	Dwarfs.
*B*0	−0.32	
*B*5	−0.17	
*A*0	0.00	
*A*5	0.19	
*F*0	0.38	
*F*5	0.62	0.62
*G*0	0.86	0.72
*G*5	1.15	0.83
*K*0	1.48	0.99
*K*5	1.84	1.26
Ma	1.88	1.76
Mb	1.88	

The correlation is so strong, in fact, that the spectral types of stars too faint to allow of a direct determination are often inferred from the colour indices. Further, the small letters, *b, a, f, g, k, m,* are used as a classification of colour index, each letter referring to the same stars as the corresponding capital in the Harvard classification of spectra. The scale of colour index has been stated, by implication, in a previous chapter. On p. 77 we explained that the magnitude scales were chosen so as to agree for certain *A*-type stars. This, of course, explains why the colour index for *A*0 stars, as given above, is 0. It will be noticed that, among the stars of later spectral type, the giants are appreciably redder than the dwarfs. This relation, however, is subsidiary to the main progression of colour with spectral type. Along both the giant and the dwarf branches, the colour index increases with

advancing type: that is the cardinal fact which must guide us later in deducing the exact physical meaning of our observations.

Colour and temperature show a closer connection with spectral type than any of the other stellar properties we have yet considered. It is clear that they are determined in large measure by the same physical conditions as those which give the distinguishing characteristics to the spectrum of a star.

CHAPTER X

STELLAR MASSES, DENSITIES AND DIAMETERS

TIME was when the mass of a body was held to be its fundamental property—or, at least, one of its two fundamental properties. Given the masses and initial motions of a set of bodies, their history, it was thought, could be predicted to the end of time. We do not go quite so far as that now. Other factors have crept into the problem, and, indeed, the very idea of mass, as the invariable quantity of matter in a body, is by no means so definite, when the whole Universe is the field of consideration, as it was once thought to be.

The fact is, the centre of attention has moved from dynamics to radiation. We no longer ask merely how much matter a star has, or how it is situated with regard to other stars. We want, even more insistently, to know how the star reacts with its immediate surroundings—the empty space that bounds it. We ask what energy it gives to space, and what it receives in exchange ; what becomes of its radiation, and how it can go on radiating through the ages of its stellar life. These are the problems that occupy the foremost place in modern cosmical inquiry.

Celestial dynamics, though assuredly a very live subject, has consequently been forced to cede a large part of the dominion over which it once held undisputed sway ; and in its decline, the importance of mass has, to a large extent, shared. But curiously enough, radiation, in its ascendancy, has discovered unsuspected potentialities in the idea of mass, and has raised it up again into a position of responsibility. Though no longer prime minister, mass at least has cabinet rank, sharing the burden of the new régime with spectrum, magnitude, velocity, parallax, temperature, colour, and density. It is as one of this group that we turn to it now, considering, as far as may be, how the masses of the stars are estimated, and, in accordance with our general plan, how they are related to spectral type.

For the direct determination of masses, we are dependent

on measurements of the gravitational attractions exerted by the stars. The dependence arises from our limitations: it is not inherent in the nature of the determination itself. If we could assume that the Universe is in what is called dynamically a "steady state," we could determine the mass of a star directly from its velocity with respect to the centre of gravity of the Universe. Again, there may be a spectroscopic effect depending on mass (see p. 41), which may, in the future, provide a means of measurement. Or, by the perfection of theoretical investigations, or even the establishment of empirical correlations between mass and other stellar qualities, yet other methods might become available. These possibilities, however, are for the future to realise or abandon.

Gravitational influences among the stars are large enough to be perceived only in connected systems—double and multiple stars. Even the moving clusters show no trace of them; their members are too far apart. Accordingly, we must restrict ourselves entirely to the double stars. The situation is not so bad as it might have been on this account, for it appears that not far short of half the stars are double. There are, however, more serious difficulties in the way, as we shall see.

When the relative motion of the components of a binary star is large enough for the relative orbit to be calculated from Kepler's laws, we can obtain the numerical value of the quantity $\mu\pi^3$, where μ is the sum of the masses of the components and π is the parallax of the star. The formula used is given on p. 109. If, therefore, we know the parallax of the star, the total mass of the system can be obtained. It must be noted, however, that we have now to pay for the benefits which the formula gave us in the determination of hypothetical parallaxes. When we assumed μ and calculated π, we could tolerate a considerable error in μ, for the resulting error in the parallax was relatively much less. But when π is known, and μ is calculated, any error in π results in a very much larger error in μ. The method is reliable, therefore, only when applied to stars whose parallaxes are known very accurately, and these are far fewer than we should wish.

If the movement of the centre of gravity of the double star can be determined, so that the absolute orbits can be calculated, it is possible to calculate also the ratio of the masses of the components, and hence their separate masses. There are not many stars for which this has been done, but the

results seem to indicate that, in general, the brighter component has the larger mass. It appears, however, that there is a rough approximation to equality in the masses of the components of these stars.

Except for special types of star, we have no other means of measuring individual stellar masses. The results given by this method are so meagre that the necessity of devising some means of extending our statistical knowledge becomes urgent. By *statistical* knowledge, we mean a knowledge of the average masses of selected groups of stars. This might be possible, just as it was possible to obtain average parallaxes of groups of stars whose individual parallaxes were beyond our reach. Let us see what possibilities there are of doing this.

We are confined, so far as we know, to the knowledge obtainable from the binary star equation given on p. 109, which we may rewrite in its usual form: $\mu = \frac{a^3}{\pi^3 P^2}$. In this equation we know, as a rule, the period, P; but $\frac{a}{\pi}$, the semi-major axis of the orbit in linear measure, we do not know, nor can we find an average value for it. There are, nevertheless, two methods by which the difficulty may be overcome.

There are a number of double stars whose components, for various reasons, cannot be seen separate. Some of these stars, however, are known to be binaries, because of the periodic Doppler oscillation of the spectrum lines resulting from the changing directions of orbital movement. To systems of this kind the above equation can be applied, and the data which the spectroscope gives reveal the dimensions of the relative orbit without calling upon a knowledge of the parallax of the star. It would seem, therefore, that the sum of the masses could be determined at once in a star of this type. But there is one thing that prevents this: the spectroscope reveals only the component of motion in the line of sight, and therefore tells us nothing about the plane in which the orbital motion takes place. Now it is absolutely necessary to know this plane in order to calculate the mass. Unless, therefore, a star is, at the same time, a "visual" and a "spectroscopic" binary—or, being a spectroscopic binary only, shows, by periodic eclipses of one component by the other, that its orbit lies in a plane through or almost through the star and the Earth—the masses of individual stars cannot

be obtained by this method. But we can use the method statistically. Taking a considerable number of stars, we can assume that the inclinations of the planes are distributed at random, and adopt the appropriate mean value. In this way we can obtain a very accurate knowledge of the mean mass of a group of stars—unless, of course, there is some unknown influence leading the stars to perform their revolutions in a particular plane or planes, for which there is no evidence at all.

It is very desirable to extend the statistical method to all classes of double stars, but the parallax difficulty stands always in the way. Seares, however, has recently developed an ingenious method by which this may be overcome. He eliminates the quantities, a, π and P from the binary star equation by the following device. Consider some particular binary star. We have the equation given above, $\mu\pi^3 = \frac{a^3}{P^2}$, where μ and π are the true mass and true parallax of the star. Now, as we saw on p. 109, if we suppose $\mu = 2$ (*i.e.*, the complete binary star to have twice as large a mass as the Sun), we obtain the *hypothetical* parallax, which we will call π_c. We have, therefore, the additional equation, $2\pi_c^3 = \frac{a^3}{P^2}$. From these two equations, we have, at once, $\mu\pi^3 = 2\pi_c^3$; *i.e.* $\mu = 2\frac{\pi_c^3}{\pi^3}$. We have here got rid of a and P, but we have still π and π_c to eliminate. This can be done by introducing the absolute magnitude. We know from p. 80 that the absolute and apparent magnitudes of a star are connected with the parallax by the equation $M = m + 5 + 5 \log_{10}\pi$; *i.e.* $\log_{10}\pi = \frac{1}{5}(M - m - 5)$. If, now, we use π_c instead of π, we have simply to substitute M_c for M, where M_c may be called the hypothetical absolute magnitude. We then obtain: $\log_{10}\pi_c = \frac{1}{5}(M_c - m - 5)$, so that $\log_{10}\pi_c - \log_{10}\pi = \frac{1}{5}(M_c - M)$. But, from the equation above, connecting μ, π_c, and π, we have: $\log_{10}\pi_c - \log_{10}\pi = \frac{1}{3}\log_{10}\frac{\mu}{2}$. Hence, $\frac{1}{3}\log_{10}\frac{\mu}{2} = \frac{1}{5}(M_c - M)$, or $\log_{10}\mu = 0.3 + 0.6(M_c - M)$.

Now, to obtain the mass, μ, from this equation, we must know both M_c and M, and since this is equivalent to knowing the parallax, we are no better off than we were before. But

if we take a large number of stars, we can write the equation in the form for averages—

$$\overline{\log_{10}\mu} = 0.3 + 0.6(\overline{M_0} - \overline{M}) ;$$

and it is now necessary to know only the average values of M and M_0 for the whole group. It is here that the value of the device is seen, for the absolute magnitude of, say, a dwarf star of a given spectral type is fairly constant, as Russell's diagram (Fig. 8) shows : the variation is negligible compared with the almost unlimited variation in parallax. Hence, all we have to do to obtain the average values of masses of stars of similar spectral type is, first, to make sure that we are selecting only groups of stars which are homogeneous with regard to absolute magnitude, and then apply the equation.

The outstanding feature of the results of stellar mass determinations is that the range of mass is very small—at least, compared with the range of luminosity. No star is known with a mass so small as $\frac{1}{10}$th of that of the Sun. Masses greater than ten times the Sun's mass are very rare. It is significant, however, that they do exist. Plaskett has recently investigated a double star (B.D. 6° 1309), the masses of whose components cannot be less than 85 and 72 times the mass of the Sun. This star, so far as we know, is quite exceptional. It is at least four times as massive as any star previously known. The general results seem to indicate that, above a certain limit of mass, a star must become unstable : theoretical work, in fact, points to this conclusion, as we shall see.

The relation of mass to spectral type can hardly be determined from the meagre data given by the direct method of measurement. Seares's statistical method, however, which can be applied to a considerable number of stars, gives the following average masses for dwarf stars of the various spectral types :—

TABLE IV

Bo	10	*Go*	1.0
Ao	6	*Ko*	0.68
Fo	2.5	*Ma*	0.59

Here the Sun's mass is taken as the unit. For the giant stars the data are very scanty, and only general conclusions can be drawn. It appears that there is a similar, though much less regular, decrease of mass with advancing spectral type. Moreover, for any given type, the average mass of the giants is appreciably higher than that of the dwarfs.

In interpreting the above table, Seares insists that the effect of selection of stars for measurement must be borne in mind. He considers that the stars used were certainly selected in such a way as to give a progression of this kind, and this may account for the whole of the apparent relationship. At the same time, it seems likely that there is actually a change of mean mass with spectral type in the direction shown by the table. According to Seares, the values for individual dwarf stars do not vary greatly on either side of the mean value, for any spectral type: probably half the stars of each type have masses between 0.7 and 1.5 times the mean. Russell has criticised the reasoning by which Seares arrived at this conclusion, but, from other directions, he has found support for the conclusion itself. It might be mentioned that several independent investigations indicate that the *B* stars are considerably more massive on the average than the other principal types.

On the whole, though conclusive evidence has perhaps not been obtained, we may probably, without error, take the order of mass of the stars to be the order of the Harvard sequence, with different rates of change along the giant and dwarf branches.

From the question of mass we come naturally to that of density. By the density of a star is meant its mass divided by its volume. Defining it in this way, we, of course, take no account of possible variations of density in different parts of the star—not because such variations do not exist, but because we have no experimental means of measuring them. Eddington's theoretical researches indicate that, if we take the ordinary laboratory definition of density—the mass of unit volume—the density at the centre of a giant star is probably more than fifty times the *mean* value. With such large variations as this implies, it will be clear that stellar density, as we have defined it, gives us very little definite information about the physical conditions in a particular part of a star, such as its atmosphere for example. It is,

nevertheless, of very great significance in comparing stars of different types.

Densities, like masses, can be determined only from binary stars—and, with the greatest amount of reliability, only from those whose orbits lie in a plane passing through or near the Earth so that we may observe the eclipse of one component by the other. The data are even more restricted than are those for mass, for, to determine density, we must be able to measure both mass and volume: there is no known method of determining the density of a star directly. If a star is an eclipsing binary, however, we have, in addition to the binary star equation, the times occupied by the eclipses, and the variations of light during their progress. It is then possible to calculate the diameters, and therefore the densities, if we know the ratio of the masses of the components. This, as a rule, is not known, but it can often be assumed from the relative brightness without considerable error.

If we knew how to measure the *surface brightnesses* of stars—*i.e.* the luminosity per unit area—we could obtain the volumes (supposing the parallaxes to be known), and hence, for binary stars of known mass, the densities. For, from the parallax, the absolute magnitude can be calculated, and therefore the total luminosity of the star. Dividing this by the surface brightness, we get at once the area of the effective surface exposed to us, and hence the volume of the star. With a binary star, it is not necessary to know the parallax if we can assume the ratio of the masses of the components. The difficulty, however, arises from our ignorance of the surface brightness.

It is a reasonable assumption—which appears to be supported by the evidence available—that the surface brightness of a star depends entirely, or almost entirely, on the "surface conditions" which we may take to be represented in some manner by the temperature, colour index, or spectral type. If this be so, the relative densities of all visual binary stars can be determined. Seares has so determined a large number, calculating the surface brightnesses of giants from the temperatures and absolute magnitudes, and of dwarfs from the colour indices, and thus obtaining mean values of density of different types of star from his statistical determinations of mass. Apart from the indications of individual stars, which may or may not be representative of their classes, these estimates form our only means of discovering, from

actual observation, the detailed relation between density and spectral type. Whatever may be their absolute accuracy—it is probably fairly high—there can be little doubt that relatively they represent the actual state of affairs, and we may consider them with confidence in the light of the Harvard classification of spectra.

The relation of density to spectral type is very remarkable. Seares's results—if we had not, thanks mainly to Russell and Eddington, been led to anticipate them on theoretical grounds—would probably have caused considerable surprise. It appears that the range of density is exceedingly large—extending from almost incredibly low values for giant *M* stars to densities more than five times that of water for the dwarfs of *M*-type. Taking the density of water under ordinary terrestrial conditions as unity, and assuming for giants a mean mass of ten times that of the Sun, the actual figures are as follows :—

TABLE V

	Density.	
Spectral Type.	Dwarfs.	Giants.
*B*0	0.04	0.08
*B*5	0.20	0.03
*A*0	0.36	0.008
*A*5	0.40	0.002
*F*0	0.40	0.0004
*F*5	0.39	0.0001
*G*0	0.68	0.00002
*G*5	1.2	0.00001
*K*0	1.3	0.00001
*K*5	1.4	0.000002
Ma	5.4	0.0000006.

Here we come across another indication that the distinction between giants and dwarfs is really a radical one. In most of our stellar characteristics we have found differences for these two groups, but, except in the matter of absolute magnitude, the differences were small and might have been attributed to secondary causes. There can be no doubt now, however, that the giants and dwarfs are two fundamentally different types of star. The Harvard sequence, in failing to

discriminate between them, has shown itself to be inadequate as a classification of stars. As a classification of spectra it is wholly admirable, but a star is more than the Harvard account of its spectrum.

As the result of our investigations, we have drawn two general conclusions about the stars as a whole: the masses are, as a rule, not very different from the mass of the Sun; and the densities have a range of about ten-millionfold. It follows that the volumes must have a wide range also. There should be stars, for example, more than ten million times as spacious as the Sun. There is, of course, nothing inherently impossible about this, but our minds are such that we should feel more satisfied if we had independent confirmation of such enormous dimensions.

This could be obtained if we could measure the diameters of the stars. Until quite recently, however, we had no means of doing this. The apparent disc of a star, enormous though the area may be, appears so small at stellar distances that it remains a point even when seen with our most powerful telescopes. But, within the last few years, by means of a remarkable application of the interferometer, the angular diameters of stars have actually been measured directly. It would take us too far afield to enter into the theory of the process. All we can do is to describe in outline the apparatus used and the character of the observations made.

The interferometer consists in principle merely of an opaque disc, which is large enough completely to cover the object glass or mirror of the telescope to which it is attached, and is pierced with two small holes whose distance apart can be varied. When it is placed in front of the telescope, and the telescope is directed towards a star, the image of the star (formed only by light coming through the two holes), is crossed by a number of parallel dark lines, known as "interference fringes." Now suppose the holes to be separated gradually. The fringes begin to lose distinctness and, if the diameter of the telescope is large enough, the separation will at last reach a value for which the fringes will disappear altogether. Let this separation be d feet. Then, if we suppose that the light from the star has an effective wave-length, λ—measured also in feet—the angular diameter of the star is given at once by the expression $1.22 \frac{\lambda}{d}$. If the distance of the star is known,

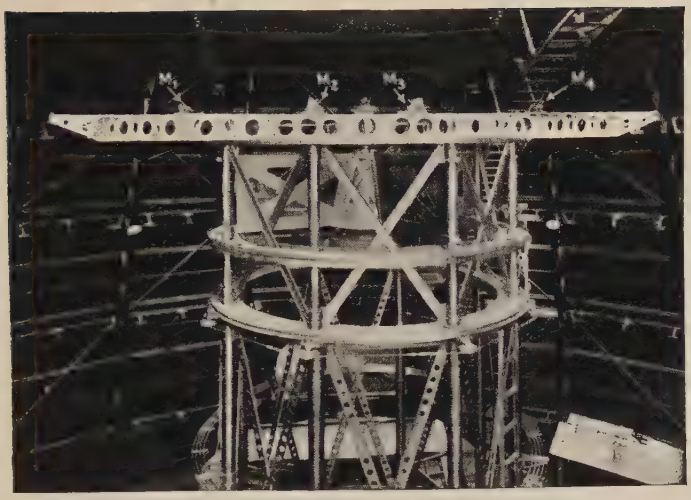
the angular diameter can be converted into linear measure.

In practice, this simple process needs a little modification. So far as we know there is no star whose angular diameter is so great as to make the fringes disappear, even when the distance between the holes is equal to the span of the largest telescope we have—the 100-inch reflector at Mount Wilson. Consequently, an auxiliary system of mirrors is employed to enable rays of still wider separation to be used. For what is essential in the arrangement we have described is that two separate rays of light from the star shall be brought together at the focus of the telescope. It matters not whether they are selected immediately by the holes in the disc, or by two small mirrors which reflect them into the holes. Accordingly, the instrument actually used at Mount Wilson has the form shown in Plate XVII. M_5 is the large 100-inch mirror, and M_1 and M_4 are the small mirrors with adjustable separation which select the two rays that produce the fringes. These rays are reflected to the secondary mirrors, M_2 and M_3, and pass thence through the holes to the telescope, producing fringes at its focus, just as if they came straight from the star through the holes.

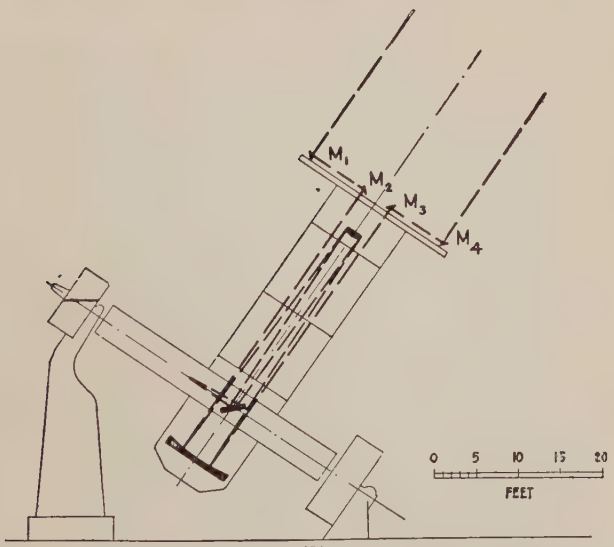
It is to Michelson that we owe, in great part, the idea of this instrument; and on the 13th December, 1920, Pease succeeded in using it to obtain the first measurement of a star's diameter. He found the angular diameter of the giant star, Betelgeuse, in the constellation Orion, to be 0″.047. The parallax of this star is approximately 0″.02, so that the linear diameter turned out to be 215,000,000 miles. Here was indeed a giant star! Since that time, the diameters of Arcturus, Aldebaran, and Antares have been measured, with results, respectively, of 21,000,000, 270,000,000 and 400,000,000 miles. There can be no doubt, then, that stars of these enormous dimensions do exist, and the name *giant* is well chosen to describe them.

The interferometer with which these measurements were made allowed a maximum distance of 20 feet between the mirrors. This is too small for any but a very few stars whose angular diameters are exceptionally large. Accordingly, a 54-feet instrument is under construction, and a special telescope is being erected to accommodate it. With this equipment it is hoped to measure the angular diameters of more than 30 stars brighter than the 4th apparent magnitude,

PLATE XVII.



(*a*)



(*b*)

(*a*) Photograph.
(*b*) Diagram of 20-ft. Interferometer attached to 100-inch telescope of Mount Wilson Observatory.

To face page 132.

covering a wide range of spectral type. It is believed, too, that the possibilities of the 20-feet instrument have not yet been exhausted. The fringes seen with it become appreciably less distinct with a number of stars when the mirrors are separated to their greatest extent, and it is hoped that the loss of "visibility" might be measured accurately enough to reveal the distance between the mirrors at which the fringes would disappear entirely.

Perhaps the chief difficulty of the method is the evaluation of the "effective wave-length" to be used in making the calculation. This is not the quantity which we have called by the same name in Chapter IX. In fact, all we can say about it is that, if the light of a star were monochromatic, it would have to be of this particular wave-length in order that the fringes should disappear under the same conditions as they actually do. With the Sun it is possible to determine this quantity, for we can measure the diameter of the Sun by independent methods, and so apply the equation given above to the determination of λ. For other stars the effective wave-length is determined from the effective temperature, suitable corrections being applied for atmospheric absorption. It is important that the error in λ should not exceed 10 or 20 A.U.

The evidence of the interferometer, then, so far as it extends, is in keeping with the indications of earlier researches, that there are stars whose dimensions are comparable with those of our own solar system. We are forced to believe also that the masses of these stars are, at any rate, not greatly in excess of the mass of the Sun, so that the matter in them must be extremely rarefied. Startling as this may seem, it is really no more than we have imagined for a very long time. Laplace's nebular hypothesis of the origin of the solar system required precisely this condition for our own Sun at a former stage of its history. We must now accept such stars as established facts, and, in seeking beneath the phenomena for the causes that underlie them, take into account a range of density among the stars of ten millionfold.

CHAPTER XI

CORRELATIONS BETWEEN STELLAR CHARACTERISTICS

We have now considered all the principal stellar qualities within the scope of our measuring instruments, and we have found that, with respect to each of them, the stars can be arranged in a definite order, more or less closely related to the Harvard sequence. We appear, therefore, to be in a position to face the grand questions: Is there a fundamental quality (or qualities) of a star, with respect to which the others may be regarded as subsidiary and capable of prediction when the fundamental quality is known? If so, what may be that quality or qualities? Finally, can we find a connecting link, not only between the different properties of the same star, but between different stars? If we can answer these questions satisfactorily, we shall have a fair understanding of the way in which Nature works in the physical world.

But before we turn our attention to these wider generalisations, it is necessary that we should know more completely how the various stellar characteristics are related one with another. We have seen already how each of them is connected with spectral type: we must now see what other relations exist.

It might seem that, in connecting every other stellar quality with spectral type, we have exhausted all possible correlations. Thus, if the order of the Harvard spectral sequence is the order of decreasing temperature, and also the order of increasing velocity, how can the orders of decreasing temperature and of increasing velocity be any other than the same, and what more is there to be said about it? The objection is natural, but it is not a valid one, for three reasons.

In the first place, we must realise that a physical correlation is something more than a mere *co-ordination*. When we say that two qualities are physically correlated, we mean that there is a direct connection between them: for example, the volume and temperature of a gas are correlated. But there are events which happen together—*e.g.*, the freezing

of a pond and the falling of the leaves of trees—which have no direct connection with each other. These events are not correlated; they are merely co-ordinated. To say, for instance, that one was the cause of the other, because they are often observed together, would be totally to misunderstand the nature of the relation between them. Similarly, we must recognise that the fact that the velocities and temperatures of the stars are both in the order of the Harvard sequence does not at all imply a physical correlation between them. We must look more closely into the other aspects of these qualities before we can come to a conclusion on that point.

Secondly, there may be a feeble correlation between two qualities, which is hidden by a strong correlation between each of them and spectral type. Thus, if we eliminate spectral type altogether, it may be that we shall find the order of decreasing temperature to be the order of *decreasing* velocity, for (leaving the distinction between giants and dwarfs out of consideration, for simplicity), in saying that the order of decreasing temperature is the order of the Harvard sequence, we do not imply, for example, that every *A* star has a higher temperature than every *F* star, but merely that the average temperature of the *A* stars is higher than the average temperature of the *F* stars. If we arranged the stars individually in the order of temperature, without regard to any other consideration, we should find a certain amount of intermingling of the spectral types in the result. The stars of any one type would cover quite an appreciable range of temperature. The same thing is true with regard to velocities. The stars having *any one spectral type* (say, the same actual stars as before), would cover an appreciable range of velocity also. Now, would these same stars appear in the same order in both ranges—the range of temperature and the range of velocity? If it is strictly true that the order of decreasing temperature is the order of increasing velocity, then they would. But they might very well do nothing of the kind, without in the least degree affecting the truth of the statement that both these orders are the order of the Harvard sequence. The correlation of both temperature and velocity with spectral type might be so strong that it quite overpowers a possible feebler correlation of temperature with velocity acting in the opposite direction. It is only when we get rid of the influence of spectrum by considering

a single spectral type that the true connection of temperature with velocity can be made clear. It should be said that we have chosen this particular case merely as an example : we do not imply that there is actually a correlation of any kind between temperature and velocity.

It would appear, then, that every possible pair of stellar qualities must be examined before we can be sure that the whole tangle of relations has been grasped ; and, in fact, to do the task completely, this is what has to be done. We shall not, however, weary the reader with a discussion of all possible correlations : we shall confine ourselves to those pairs of qualities between which strong correlations have already been established.

The third reason invalidating the objection referred to above is this : no scientific conclusion, however well supported, can afford to neglect a single chance of confirmation or disproof. This is true generally : especially is it true of the particular methods of correlation that we have to adopt in dealing with the stars. There are, as we know, thousands of millions of stars : we know the spectral types of not more than 250,000, and our data for other characteristics is generally still less extensive. We have to determine the laws of the stellar universe from a tiny fraction of one per cent. of the stars.

And this is not the worst. We have seen that we are limited in our investigation of each characteristic by conditions imposed by the nature of the problem : thus, only industry and patience are required to give us the apparent magnitudes of all the stars we can detect, while for radial velocities we are restricted to stars whose luminosity is great enough, and whose spectral lines are sharp enough, to allow us to measure the Doppler displacements. Consequently, the stars from which our data for the various qualities are drawn are, in general, different stars in every case. In attempting to correlate the results, therefore, the possible effect of selection must be eliminated first of all.

This question of selection is so important that we must give it some attention before proceeding further. We have said that we can detect thousands of millions of stars. There are, without doubt, large numbers that we cannot see because they are too faint or too far away to be within reach of our telescopes. At the very beginning, therefore, we must recognise that if we could deal always with every known

star, we should be considering only a selection characterised by nearness or brightness.

Of course, we cannot deal with them all. Even if we could get all the data, the very enormity of the task would prohibit results within a reasonable period; and we know that the full data are not available. Kapteyn attempted to minimise this difficulty by nominating 206 "selected areas," which were chosen so as to be as representative as possible of the whole sky, so far as he could judge. He proposed that observers of different stellar characteristics, at different observatories, should confine their attention to these particular areas, so that the results, when they came to be compared should refer, as nearly as possible, to the same stars. Kapteyn's suggestion has been generally adopted, doubtless with great advantage, but the difficulty cannot be completely removed in this way. We may consider stars in the same parts of the sky, but the selection in those parts must depend on the character of the particular problem we are attacking.

It follows that there may be correlations between stellar qualities that we do not discover because our data are not sufficiently homogeneous. It is much more likely, however, that we shall discover correlations which do not exist—which arise entirely from the peculiarities of our selections. For example, suppose we could measure the distances and absolute magnitudes of all the stars we can photograph. Since the intrinsically faint stars can be detected only when they are relatively near to us, while the intrinsically brightest stars can be detected, not only near at hand but at the farthest distances to which we can penetrate, we should find that the average brightness of the stars grew greater with increasing distance. If we accepted this fact without regard to the conditions of observation, we should conclude that we had found a correlation between absolute magnitude and distance from the Sun, and proceed to build up a theory of cosmogony accordingly. But we can see by reflection that, whatever may be the distribution of stars in space, we must necessarily arrive at this result, because of the limitations inherent in the nature of observation itself. The apparent correlation would have no basis in fact.

Again, before Adams introduced the spectroscopic method of determining absolute magnitudes, measures of stellar parallax were, for the most part, confined to those stars whose large proper motions indicated that they were probably near

enough to give a measurable trigonometrical parallax. But large proper motion might arise also from a large space velocity, so that there would be a possibility that the stars whose parallaxes were measured would, on the average, have a speed higher than the average speed of all the stars. This would not necessarily be so—the large proper motions might be due entirely to nearness—but it would be possible. Consequently, if a comparison were made between the velocities of these stars and those of the more distant stars whose parallaxes were measured later by the spectroscopic method, and if it were found that the former velocities were greater, on the average, than the latter—we should not know, from these facts alone, whether the result were due to the method of selection of stars for trigonometrical parallax measurement, or to an actual correlation of velocity with distance, by which the distant stars move more slowly, in the mean, than the near ones.

It will be realised, then, that before conclusions can be drawn from apparent correlations between the various stellar characteristics, the possible effects of selection must be given very careful consideration. One way of testing the matter is to see if cross correlations hold : for example, if *a* appears to be correlated with *b*, and also with *c*, we can see whether or not there may be a contradictory correlation between *b* and *c*. This is another reason, then, why we should not neglect the investigation of all possible correlations between stellar characteristics. We will now consider briefly the outstanding correlations that have so far come to light.

To begin with what is probably a dynamical phenomenon—there appears to be a distinct correlation between mass and velocity, such that, the greater the mass of a star, the more slowly it moves. Seares finds this correlation to be shown very strongly for dwarf stars by his statistical results, and he has shown, in fact, that its character is such that the kinetic energy, which is measured by the quantity $\frac{1}{2}mv^2$ (m = mass, v = velocity), is practically the same for all dwarf stars.

Now this is an exceedingly important result, because uniformity of kinetic energy—" equipartition " of energy, as it is called—is a necessary condition that the universe of stars shall be in a dynamically " steady state " : that is, that the stars shall have been moving about long enough for all their gravitational interactions to have produced their full effect.

The existence of stream motion, however, shows that the steady state has not actually been reached : all we can say is that we are well on the way towards it. The generally accepted view that the Universe is exceedingly old is supported by this result. It is, perhaps, significant that the *B* stars are an exception to the equipartition correlation, as they are to other generalisations to which the remaining types contribute. They are decidedly the most massive of the stars of the chief spectral types, but their velocities are so small that their mean kinetic energy is much below the value [1] for the other types.

It should be mentioned that, in obtaining his results, Seares did not remove the effect of stream motion from the velocities. Had he done so, it is probable that he would have found a still closer approach to equipartition. It may be possible in the future to make use of this correlation in order to determine how nearly our practical standard of rest conforms to the ideal—the centre of gravity of the Universe. For equipartition is a gravitational phenomenon, and it will hold only when the velocities are measured with respect to the centre of gravity of all the gravitating bodies. Consequently, if we determine a standard of rest such that the velocities of stars, when referred to it, will give uniformity of kinetic energy, then, granting the reality of equipartition, that standard must be the actual centre of gravity of the Universe. The fact that equipartition has already been found to hold in the mean for the different types may seem to show that the standard by which we measure velocities now is actually the ideal one. This, however, is not necessarily so, for, since each type comprises stars moving in all directions, the addition of any constant velocity at all might, on the average, increase the kinetic energy of half of the stars as much as it might decrease that of the other half, and the mean kinetic energy for the type would then be the same as before. The method must be applied to individual stars if it is to be of any use ; and, since gravitation takes no notice of spectral type, or indeed of anything but mass and motion, we need not be restricted by any consideration at all when we have assured ourselves that the stars used belong to the groups for which equipartition is established. Moreover, gravitational influence being effective at all distances from the

[1] The mean kinetic energy of translation for stars other than those of type *B* is of the order of 3.5×10^{48} ergs.

gravitating bodies, we are automatically considering the effect of the whole Universe, and consequently are untroubled by the effects of selection.

A very important feature of this correlation is that it seems to offer an explanation of the apparent relation between velocity and spectral type. It has always been difficult to understand how any close connection could exist between two such dissimilar qualities. It now appears to be a result of the correlations between mass and velocity on the one hand and mass and spectral type on the other. We have seen that mass decreases with advancing spectral type, along either the giant or the dwarf branch. Hence, velocity must increase with advancing spectral type, if there are no disturbing influences. Moreover, the average masses of giants are greater than those of dwarfs of the same types. The corresponding velocities are therefore smaller.

The explanation appears to be complete, qualitatively: there are not sufficient data to test it for individual stars. It seems, however, that in summing up our knowledge of the stars, in order to form generalisations with regard to the progress of Nature, we can dispense with a direct connection between velocity and spectral type. Mass and velocity, mass and spectral type—these appear to show the fundamental correlations, and we shall make this assumption when we come to summarise our evidence and see the direction towards which it points.

Next to spectral type, absolute magnitude seems to show the greatest tendency to connect itself with other stellar qualities. It is conspicuously correlated with space velocity, mass, density, and colour. The correlation between absolute magnitude and space velocity has emerged from a rather lengthy discussion arising from the observation that the radial velocities of stars increase with the proper motions. A large proper motion probably indicates nearness. The nearer stars are, on the average, intrinsically fainter than the known distant ones, which must be bright in order to be seen. Hence, large proper motions denote, on the average, relatively faint stars, whose high radial velocity is simply the result of a general increase of space velocity with increase of absolute magnit eud.

The same phenomenon appeared strongly, though less convincingly than it is now seen, in an earlier study of the

stars nearest to us—the stars within a sphere of radius 5 parsecs. These stars, which numbered nineteen, were found to have an average velocity much greater than the average velocity of all the stars. The reason was that they included some intrinsically very faint stars, which could be detected at their comparatively short distance from us, whereas stars of equal absolute brightness would be invisible among the more distant stars. These stars, having large velocities, increased the average velocity of the nearer stars, and made it higher than the general average.

Adams, Stromberg and Joy have made the latest contribution to this inquiry. They have shown that there is a very regular increase of space velocity with decreasing brightness among the giant stars of types *F*, *G*, *K*, and *M*. The dwarf stars, however, have a much greater range of velocity than the giants, and the individual peculiarities of motion among them are so great as, to a large extent, to conceal the relation, if it exists. A decrease of one magnitude in brightness among the giant stars corresponds to an increase in average space velocity of about two miles per second. With regard to motions, the giant stars appear to form a much more homogeneous class than the dwarfs, which show an abnormal percentage of very high velocities (see p. 101).

Can we regard these facts as establishing a direct physical correlation between velocity and absolute magnitude? It is very difficult. Absolute magnitude, like spectral type, is an absolute property of a star, while velocity is relative to an arbitrary standard of rest. It is very hard to believe that if, say, half of the Universe were suddenly moved with respect to the other half, its stars would automatically grow brighter or fainter. We must look for some other correlation of which this is a symptom.

Mass again comes to our aid. It seems likely that there is a correlation between mass and intrinsic luminosity which, combined with the mass-velocity relation, gives the effect we are considering. Seares has given the most complete expression to this correlation, as the result of his statistical investigations. He finds that, within the same spectral type, mass, on the average, increases with absolute brightness. This result is not finally established by his researches, because in deriving it he assumes that the kinetic energies of the stars are the same whatever their magnitudes, whereas he has only shown that the kinetic energies do not vary with spectral

type. It is, however, extremely probable that it represents the facts. Van Maanen, a little earlier, found a correlation between mass and luminosity, and Seares's result may be regarded as a more comprehensive statement of the connection.

We shall assume, therefore, that the magnitude-velocity relation is an effect of two correlations—between mass and velocity, and mass and absolute magnitude, respectively. This, at least, seems reasonable, and is capable of an intelligible interpretation. A direct connection between velocity and absolute magnitude would be a *cul-de-sac*. We should have to accept it as an ultimate fact, not to be explained in terms of previous knowledge.

Magnitude and density show a correlation that offers no such violence to our common sense. This connection is brought out most conspicuously in Seares's investigations. We have already seen that absolute magnitude and density are the two qualities in which the distinction between giant and dwarf stars is most completely expressed. We shall not be surprised, therefore, to find that they run along parallel lines: the fainter the star, the greater its density. This applies to giants and to dwarfs—the sequences of both absolute magnitude and density seem to run continuously through the giants into the dwarfs. For the giants, Seares gives the following formula, connecting density with spectral type and absolute magnitude. If ρ represents density, M, absolute magnitude, and S, spectral type (counting *B*0 as 0, *A*0 as 1, etc.), then $\log_{10}\rho = -1.22S + 0.57M + 1.11$. Density and brightness have evidently a very close connection one with another—a conclusion which is well supported for the comparatively small number of stars for which it has been possible to make individual measurements.

There is a considerable amount of evidence pointing to a connection between apparent magnitude and colour. Seares has shown clearly that, in two widely different regions of the sky, as the apparent brightness diminishes, the stars, on the average, become redder. Now this can be explained in at least two ways: the distant stars may be redder than the near ones, or the intrinsically brighter stars may be redder than the intrinsically fainter ones. The second possibility is, perhaps, not immediately obvious, but it comes about in this way. The apparently faint stars include those which are

absolutely faint, and those which are absolutely bright but are at great distances. The latter greatly outnumber the former, for they are distributed through a much greater volume of space. Hence, apparently faint stars include mainly distant bright ones.

It has probably not yet been finally settled how far the effect is due to one cause and how far to the other. A third possible explanation is that, in the regions of the sky examined by Seares, the limits of the stellar universe are so near that the intrinsically very bright stars are not distant enough to be included among the apparently faint ones. If this were so, we should not find blue stars among the stars of smallest apparent brightness, because blue stars (types *B* and *A*) are all intrinsically bright—the giant and dwarf distinction is very inconspicuous among them (see Fig. 8). The redder stars, however, may be intrinsically bright or intrinsically faint, for among these stars the giants and dwarfs are widely separated in absolute magnitude. Consequently, the apparently faint stars would be, on the average, redder than the apparently bright ones.

Whatever may be the chief connecting link between apparent magnitude and colour, however, there is certainly a relation between absolute magnitude and colour. Hertzsprung found a regular increase of effective wave-length with apparent brightness for the stars in the globular cluster, N.G.C., 1647. Now the stars in a cluster of this kind are approximately at the same distance from the Earth, so that their apparent magnitudes are a direct measure of their absolute magnitudes. Consequently, Hertzsprung's observation was direct evidence for the continuous reddening of stars with increasing absolute brightness, supposing, of course, that the cluster stars are typical of the whole firmament.

Let us now review the conclusions we have reached in the chapters of this section. We have considered eight qualities of the stars—their spectra, magnitudes, movements, distances, temperatures, colours, masses, and densities. Of these there are four—spectra, temperatures, colours, and masses—whose general order of change is the same—that of the Harvard sequence, with secondary distinctions, however, between giant and dwarf stars. In two of the qualities, on the other hand—density and absolute magnitude—the giant and dwarf distinction is at least as pronounced as the distinction

between the various Harvard classes. The order of increasing density is *gM*, *gK*, *gG*, *gF*, *gA*, *B*, *dA*, *dF*, *dG*, *dK*, *dM*, where *g* stands for "giant" and *d* for "dwarf." Though there is not much variation of absolute magnitude along the giant branch, we may take this, without appreciable error, as representing also the order of increasing absolute magnitude. The order of distance is somewhat ambiguous, but seems, on the whole, to be related to the Harvard sequence, if at all, in a rather complex manner. The balance of evidence is against any direct relation between the Harvard sequence and individual stellar velocities.

Velocity, however, is not an isolated quality: it comes into relation with mass, giving an approximation to equipartition of energy. Further, colour and absolute magnitude are related, the brighter stars being redder than the otherwise similar fainter ones. There may also be a relation between colour and distance. Finally, there may be some significance in the fact that, while the range of mass, temperature, and colour among the stars is not very great, there is an enormous range of density and absolute magnitude. Whenever a quality has a large range of values, it shows the giant and dwarf distinction strongly.

These are the facts, out of which it is the task of the astrophysicist to build up an ordered system. We must seek for a "cause and effect" relation, so that we can say, for example, what particular stellar qualities determine the spectrum, or the temperature, or the magnitude. From empirical correlations we must deduce fundamental connections, working back from the many qualities into the few or, if possible, the one. It is our purpose now to see how far this can be done.

CHAPTER XII

STELLAR EVOLUTION

When a number of phenomena are interrelated, the simplest possible expression of their connection may be made in one of two ways. It may be that one (or more) of them is more fundamental than the others, and that its variations are directly responsible for the variations of all. For example, the motions of three billiard balls, and the indications of the scoring board, are all expressible in terms of the motion of the ball struck by the cue. But sometimes an explanation of this kind is not possible. To extend an example cited in the last chapter—the falling of the leaves of trees, the freezing of a pond, the shortening of the days, the "deep, autumnal tone" of the west wind—there is none of these that can explain the others. To account for their concurrence, a new phenomenon must be introduced—the changing declination of the Sun. From this they follow as parallel but independent consequences.

In the case before us—the multitudinous characteristics of the stars—the data seem to be too complex for either of these types of generalisation to be adopted exclusively. To a certain extent we employ the first: thus, to anticipate, we shall account for velocity in terms of mass, and spectrum almost entirely in terms of temperature. But that is not sufficient; in particular, it offers no shadow of explanation why the spectra of two stars should be almost identical, while the luminosity of one is more than a hundred times that of the other. To express this, and other remarkable facts, in the most compact way, we introduce a new idea—that of *age*; in other words, we assume that the stars are in course of evolution.

When a body—for example, a living organism or a star—has undergone no visible change in the period of time during which we have been able to study it, and we cannot reconstruct its past history on grounds independent of our present observations of the body, two conditions must be satisfied before we can profitably assume that it has undergone any

change at all. First, there must be a large number of such bodies, showing different external characteristics; second, those external characteristics must be capable of arrangement in a continuous sequence, so that examples of each stage of the sequence may be drawn from the bodies. When these conditions exist, either completely or to a high degree of approximation, we say that the present state of each body represents, in its main features, a stage in a history which is common to all.

It must be emphasised that the idea of evolution—though, in the circumstances we have mentioned, its truth is exceedingly probable—is primarily the source of a convenient method of co-ordinating facts rather than a dogmatic statement of what actually has happened in the past. We do not speak of the evolution of the ether, for example, because, to the best of our knowledge, there is only one ether. We do not speak of the evolution of the electron, because, although there are countless millions of electrons, they are all, so far as we can tell, in the same state: they have no differing external characteristics. Fundamentally, there is no argument whatever against the idea that the ether is slowly but continuously changing its properties—the velocity of light may be changing—or that electrons are doing the same, but, being simple, almost ultimate bodies, were all born at the same time and are progressing at sensibly the same rate. So far as actual truth is concerned, an evolving ether or an evolving electron might just as well be a fact as an evolving organism or star. We do not take the possibility into account, because the assumption is of no use to us.

Advancing from these general propositions to the actual stellar data that we have been considering, we see at once that those data satisfy the conditions necessary for the usefulness of the assumption of evolution. There are millions of stars, and in almost every characteristic they show a continuously graded variation. If the sequence of changes had been the same for all characteristics, the problem of determining the order of evolution would have been very simple. We should simply have had to assign arbitrary "ages" to the various positions in the sequence, and the statement of the age of a star would then have been sufficient to describe the star completely. But we have seen that the facts are much more complex than this. There are at least two sequences—the Harvard sequence, and the order of absolute magnitude

and density which is composed of two Harvard sequences in opposite orders. Which of these, if either of them, is to be chosen as the order of evolution? And, the choice having been made, how are the various correlations that we have noticed to be explained as necessary consequences? These are the questions we have now to face.

Had we been writing a novel, we should have been tempted to preserve this chapter until the end, for the reader will find in it a solution to many of the difficulties which have possibly arisen in his mind. It is necessary, however, that, before exploring in more detail the strange storehouse of almost unimaginable physical processes and conditions that we call a star, or the perhaps still stranger activities of the nebulæ, we should have some idea of the pre-established harmony to which the phenomena contribute according to their kind. Moreover, it is not to be anticipated that every mystery is now to be cleared up. There will be much that is unexplained, and, as we proceed in the subsequent sections to closer investigations, we shall find no lack of mysteries which offer no hint of a solution. The demands of the novel reader are exacting. A Sherlock Holmes, baffled by half his cases and giving only a partial solution to the other half, would never have caught the public fancy. But that is just the position we are in now. We therefore do not hesitate to pass judgment, to the best of our power, on the facts already before us, knowing that, while we have no right to expect our decision to be final, it will nevertheless give us a point of view from which to face the new facts that will arise.

Before, let us say, the application of Adams's spectroscopic method of determining absolute magnitude, there was a widespread conviction that the order of stellar evolution was the order of the Harvard sequence. The distinction between giant and dwarf stars was then not sufficiently clear to be generally admitted; there was scarcely any knowledge of stellar masses or densities; and the order of absolute magnitude was somewhat ambiguous. The Harvard sequence, it seemed, was the order of spectra, of temperature, and of velocity: there was no characteristic of the stars that appeared to change in any other order. The evidence, in fact, seemed to point unanimously to the Harvard sequence as the order of development of the stars.

The question, of course, arose: in which direction do the

stars change—from *B* to *M* or from *M* to *B*? There were two reasons for supposing that the change was from *B* to *M*. First, it seemed more likely that a star would be born hot, and gradually cool, than that it would be born cold and gradually become hot. As a matter of fact, a star must be both losing and receiving heat. It must lose by radiation, and we are bound to suppose that there is a supply from some source in order to explain the maintenance of the Sun's heat throughout even the periods which geology requires for the Earth's existence as a habitable globe. We have no certain means of estimating the relative rates of loss and gain, but since radiation increases as the temperature of the radiating body rises, no *constant* supply of heat can possibly bring about a permanent state of rising temperature. The continuous effective-temperature change along the Harvard sequence was therefore taken as an indication that the stars change from the *B* to the *M* state.

The second reason for this conclusion lay in the relation of the stars to the nebulæ. There is an almost instinctive belief that the primal state of a star is a nebula—a belief which found its first precise expression in the nebular hypothesis of the origin of the solar system. It is hard to believe that, if the nebular and stellar states are two events in the history of the same material, the former can follow the latter. In looking for the stars of the earliest type, we look for those which appear to be nearest the nebular state. Now the early *B* stars and the diffuse nebulæ seem to occur together in space. When a star appears to be in the midst of a diffuse nebula, it is almost certainly a *B* star (see the Pleiades, Plate XVIII). Moreover, we have seen that the spectral sequence is probably from *P* through *O* to *B*. Consequently, it was concluded again that the *B* stars are young and the *M* stars are old.

For a time the growing knowledge of fresh facts seemed to give unqualified support to this theory of stellar evolution. The evidence of the double stars claimed great respect. It was generally believed that the majority of double stars originated in the division, or *fission*, of a single star into two parts. Now the visual binaries—those whose components can be seen separate—were overwhelmingly more numerous in classes *A* and *F* than they were in the other classes. The bright-line spectra had none: classes *M* and *N* claimed a doubtful one. The *spectroscopic* binaries, on the other hand

PLATE XVIII.

The Pleiades.

To face page 148.

(see p. 231), were most numerous in class *B*, and became continuously fewer among the later types.

If the order of stellar evolution is the order of the Harvard sequence, these facts have a very simple explanation. Suppose fission takes place early in a star's career, say in class *B*. Then, at first, the components will be so close together that they will generally not be visible as two distinct bodies. But they will then be moving with their greatest relative speed, so that they will be in a particularly favourable condition for detection as spectroscopic binaries. We should, therefore, find, in class *B*, few visual, but a majority of spectroscopic binaries, which is just what actually occurs. Owing to tidal influences, the distance between the components increases with time, up to a certain limit, so that we should find the majority of visual double stars in the class in which the components are far enough apart to be seen but not too far apart for the system to be recognised as a double star. This might very well occur in classes *A* and *F*, so that the facts were in excellent agreement with the proposed theory of evolution. The very small number of visual binaries in class *M* was rather surprising, but it must be remembered that, if these stars are very old, the fainter components may, in many instances, have become invisible. There was, therefore, no difficulty at all in accounting for the observations.

Another fact also, the force of which we shall be able to appreciate better at a later stage, was in harmony with the theory. The distribution of the stars in space showed a gradual change, in the order of the Harvard sequence. The diffuse nebulæ are almost exclusively confined to the plane of the Milky Way: their "galactic concentration," as we say, is very high. In passing through the *O*, *B*, and the following types, down to the *M* class of stars, we find the galactic concentration getting continuously lower and lower: the *M* stars appear to be distributed almost uniformly in all directions.

There may be more than one explanation of this, but if the proposed order of evolution is correct, there is no difficulty whatever in suggesting a very probable one. The stars, we must assume, are born in the galactic plane, where their parents, the diffuse nebulæ, are found. In their childhood, when their velocities are small, they remain at home, or near it, and so the early type stars have a high galactic concentration. As they grow older, and move more quickly, they wander

farther and farther from their birthplace until, when they have reached old age in class *M*, they may be found in any direction.

The relationship between age and velocity, it may be remarked, was always something of a mystery. No perfectly satisfactory reason could be offered of the increase of speed with age. That, however, told in no way against the theory. It was accepted, rather as a fact whose explanation, so far, stood outside the theory than as evidence either for or against it. For the observation from which it sprang—the relationship between velocity and spectral type—was itself incomprehensible. Until that was explained—and about the fact, apart from its physical meaning, there could be no doubt—no one could impeach the theory on the ground that it required fantastic things.

Taking everything into consideration, then, a very strong case was made out for the idea that the stars develop in the direct order of the Harvard sequence. As a working hypothesis—and even as something more—that idea claimed almost universal assent. But, from among recognised workers in Astronomy, there arose two dissentient voices—and only two. Sir Norman Lockyer, many years ago, maintained that the stars could be arranged in two groups—containing, respectively, stars whose temperatures were rising and those whose temperatures were falling. He was guided to this conclusion by certain spectral characteristics, which had nothing to do with the more recent giant and dwarf distinction but which pointed to an equally sharp division of the spectral types. Lockyer held that a star was born of a diffuse nebula —which he regarded as a cool, meteoritic swarm—and spent its childhood in class *M*. Its early history would be marked by a condensation which, from theoretical considerations, would bring about a rise of temperature. The star would therefore mount up the Harvard sequence until, at the top, it reached the maximum temperature which increasing radiation would make inevitable. It would then descend again, passing through the successive spectral types a second time, and finally give up its luminosity after leaving class *N*.

Lockyer supported his views with a considerable amount of experimental evidence, but, owing to the remarkable success of the more popular theory, he stood alone in their defence. Russell, however, along totally different lines, was

gradually drawing towards similar conclusions. He was greatly impressed by the relationship between spectral type and absolute magnitude—it was Russell, in fact, who first prepared a diagram of the kind shown in Fig. 8. At that time he had considerably fewer measurements of absolute magnitude that we have now, and the distinction between giants and dwarfs, shown so obviously by Fig. 8, was of a much more dubious character. Nevertheless, from the evidence of his inquiry and the measurements of stellar densities which it induced him to obtain, Russell concluded, in agreement with Lockyer, that a star passes twice through the same spectral type—first, when its temperature is rising, and second, when it is falling. His division of the spectral types into two groups, however, differed fundamentally from Lockyer's.

In presenting Russell's theory, which may now be said to have been received into general favour, we shall in this chapter say nothing of the strong support it receives from Eddington's subsequent mathematical researches. It will be sufficient at this stage to show how the theory arises from and interprets the correlations between stellar characteristics that we have described. In brief, Russell, following the universal tradition, assumes that a star arises from a very rarefied gaseous mass. In its first stage as a luminous star, it is a giant of class *M*—the most diffuse of all types of star. The order of evolution Russell takes to be the order of increasing density—that is, a star contracts throughout its life ; and he appeals to theory to show that, so far as we can predict the consequences of this assumption, they are in agreement with observed facts.

Let us trace the successive stages in the history of a contracting star. At the beginning, we may note that contraction is an eminently reasonable supposition, for gravitational influences are always at work, and if they are powerful enough to hold a star together through whatever changes take place in it, we can hardly doubt that they will cause it to contract, at however slow a rate. Supposing, then, that a young star is an extremely rarefied mass of gas, what will happen as it shrinks ? This question was answered some time ago by Lane, Ritter, and others, and their conclusions have been generally accepted. So long as the density is very small, so that the laws of a perfect gas are followed (*i.e.* so long as the average distance between two neighbouring molecules is very

large compared with the size of a single molecule), contraction will bring about a rise of temperature in such a way that the temperature at the centre of the mass increases at the same rate as the radius decreases. The temperature will not be so high in the outer layers as it will at the centre, because loss of heat by radiation takes place mainly at the surface, but it will rise with contraction, nevertheless, though at a slower rate. The "effective temperature" of the star, therefore, whatever it may be, will rise with contraction in the early stages.

But a time will come when the density is too great for the perfect-gas laws to be applied. This will occur first at the centre, where the density is always greatest, and somewhat later in the outer layers. The result will be that the temperature of the star will cease to rise and will begin to fall—first at the centre, and later in the external parts. As contraction proceeds, the temperature will continue to fall until, at last, the star degenerates into a cold, dark, dense body, and swims out of our ken altogether.

What, now, will be the value of the temperature at the critical stage when it reaches its maximum value? That will depend on the mass of the star. Other things being equal, the larger the mass, the higher will be the temperature it can attain through contraction. It follows that, since the stars have various masses, they will not all have the same history. Some will have a larger range of experience than others, and probably a longer life.

These are the requirements of dynamical theory: what are the facts? We have seen that the order of increasing density is the order, *gM*, *gK*, *gG*, *gF*, *gA*, *B*, *dA*, *dF*, *dG*, *dK*, *dM*. This, then, according to Russell, is the order of evolution of a massive star. We say "massive," because the lighter stars will not reach the *B* stage. They will turn back at *A*, or an even earlier type. An extremely light star might even be unable to get hot enough to give any light at all—a conclusion which gives a ready explanation of the fact that we find scarcely a star so light as one-tenth of the mass of the Sun. But, to include as large a range of conditions as possible, we will consider a star which reaches its prime in type *B*, and see how the history of such a star, according to Russell, is in agreement with observation.

To begin with temperature, we have seen in Chap. IX. that the facts are in general agreement with the theoretical

demands. The order of increasing density is, from observation, the order, first of increasing, and then of decreasing temperature. The observed difference between the temperatures of giants and dwarfs of the same spectral type is an effect which it is, perhaps, hardly possible to predict with certainty in the present state of knowledge. Spectral type is determined by the atmosphere of a star, and the effective temperature by the interior source of continuous spectrum. Until we know exactly what the latter region may be, and how its conditions are related to those of the atmosphere, we cannot say how secondary effects such as this may arise. It must be remembered that the Harvard classification is a classification of certain specified characteristics of spectra, and, in the strictest sense, is nothing else. Though, for convenience, we talk of a *B star*, or an *A star*, we must not be misled into thinking that we are thereby specifying anything more than the Harvard criterion of its spectrum. In particular, the only thing that is necessarily common to, say, a *gK* and a *dK* star, is the feature of their spectra selected by the Harvard observers as the basis of spectral classification. We have no *a priori* grounds whatever for expecting their effective temperatures to be the same. Indeed, we do not know—nor does it seem inherently probable—that the effective temperatures of a *gK* and a *dK* star are the temperatures of similarly situated layers. All we know is that they are the temperatures which a perfect radiator would have if it gave continuous spectra identical with those of the stars.

We can say, however, that the order of change of temperature from type to type is the order which Russell's theory requires, and that there is nothing in the observed facts with regard to temperature that is known to be inconsistent with the theory. We may now ask how it comes about that stars so widely separated in age and density as a *gK* and a *dK* star have almost identical spectra. Russell's reply is that, as Lockyer insisted long ago, the most influential agency by far in determining the line spectrum of a star is the temperature of the star's atmosphere. This is not, as we have just taken great pains to point out, the effective temperature of the star, nor do we know the exact relation between it and the effective temperature. There can be no doubt, however, that the relation is a fairly close one; and if we accept Russell's contention, we cannot question that the observed order of effective temperature is a complete explanation of

the repetition of the spectral types in the second portion of a star's life. It may be added that both laboratory experiments and modern spectroscopic theories agree in support of Russell's assumption concerning the influence of temperature on stellar spectra. The spectrum of a star is probably not determined completely by temperature, but temperature is almost certainly the predominating influence.

We have found the theory to hold, then, to this extent: a continuously increasing density would result, first in an increase, and then in a decrease of temperature, with a concomitant ascent and descent of the Harvard sequence of spectra. All this is in accordance with facts. What, now, of the other stellar characteristics—absolute magnitude, for example? Absolute magnitude depends on two factors—the brightness of each unit of area of the star's disc and the total area of the disc. Now the former factor depends on temperature: the hotter a luminous body is, the brighter, in general, it will appear. The second factor depends on the diameter of the star. Throughout the giant stage, these two factors are, obviously, opposed one to another. The star is getting hotter, and therefore brighter per unit area: and, at the same time, its diameter is shrinking, and the total area is therefore diminishing. Without a knowledge of the relative rates of the two series of changes, we cannot say what the variation of absolute magnitude will be. But it is very probable that the variation will not be great: each effect will tend to neutralise the other, and we shall not expect a large range of absolute magnitude among giant stars—at any rate, among those of comparable mass, which, for the most part, are the ones we observe.

Along the dwarf branch, however, there will be large variations of absolute magnitude. Here a star is cooling and shrinking at the same time, so that the absolute magnitude will rise on account of both decreasing surface brightness and decreasing area. Taking the stars as a whole, therefore, we should expect to find absolute magnitudes which are fairly constant among the giants and rise rapidly from type *B* to type *M* among the dwarfs. In view of Fig. 8, it is unnecessary to point out how true to fact in general this is. It was this relation, as we have said, that led Russell, in the first place, to the idea of his theory. We may remark, however, that the scarcity of *F* and late *A* type giants—which may be due to a discontinuity in absolute magnitude among

the giants at this stage—is not accounted for in this simple way. The difficulty might be overcome by supposing that the scarcity of these giants indicates that a relatively very short time is spent by a star in this stage of its development, but we shall come across evidence (see p. 173) that there is actually a discontinuity in absolute magnitude.

There is no need to explain how the changes of colour with age required by the theory are consistent with observations. Colour is so closely allied to temperature that the explanation of one is the explanation of both. We must consider the question of mass, however, for the data there are anything but obvious consequences of the theory; though, when the matter is carefully considered, they give the theory strong support.

We have seen that the general result of the investigations that have so far been made is that the mean masses of the stars diminish from *B* towards *M*, particularly among the dwarf stars, and that the mean masses of giants are greater than those of dwarfs in the same spectral type. If this is so, it is a fact that can hardly be explained by the older theory of stellar evolution. We should have to assume either that a star radiates matter as well as energy, or that, with the progress of time, the stars tend to be more and more massive when they are born. Each of these explanations savours of special pleading. According to Russell's theory, however, the explanation is completely natural, and, indeed, inevitable.

If we consider the dwarf stars of any spectral type, say *F*, we must recognise that, with regard to mass, they are a very miscellaneous set of bodies. They include stars whose maximum temperatures have been reached in the *B*, *A*, and *F* classes—that is, stars of a considerable range of mass. Dwarf stars of *G* type are a still more heterogeneous collection. They include, not only stars which have attained to the *B*, *A*, and *F* classes, but also the stars of smaller mass which have not been able to rise higher than class *G*. The average mass of the *G* dwarf stars will therefore be lower than that of the *F* dwarf stars, because of the addition of this set of lighter bodies. A similar remark, of course, applies to the other types, so that we should have, on the average, a steady decrease of mass among dwarf stars as we proceed from type *B* to type *M*.

The greater masses of the giants as compared with the dwarfs of the same type can be equally well explained. Consider a late class, say *K*, where the distinction is well marked.

The stars recognised as giants are only those which are destined to rise much higher in the temperature scale. For although, from an evolutionary point of view, we regard a *giant* star as one whose temperature is rising, we can in practice distinguish as giants only those whose absolute luminosities are great. A *K* star whose temperature is rising to a maximum which it will reach in class *G*, will appear among the dwarfs in our records, for its small mass, and consequent small surface area, give it a luminosity comparable with that of a much more massive *K* star on the descending scale. It follows, then, that the average mass of the stars *observed* as giants will be greater than that of the stars observed as dwarfs, and there is no necessity whatever to suppose that a particular star, in developing from the giant to the dwarf stage, undergoes any loss of mass.

We have now covered the whole of the correlations explicitly mentioned in Chapters V. to X.—the correlations, namely, of the various stellar characteristics with the Harvard sequence of spectral types. We have found that Russell's theory offers a satisfactory account of them all. There remain the further correlations brought out in Chap. XI.

The first of these—the connection between velocity and mass—really stands outside a theory of evolution. It is a dynamical necessity, independent of age—except that the close approximation to equipartition of energy that we observe shows that the Universe must be very old. From this point of view, indeed, we may say that the correlation helps to justify a tacit assumption inherent in any theory of stellar evolution based on a change from one spectral type to another : it provides the necessary time. The colours of the stars—and therefore, presumably, their spectral types—have not sensibly changed in historic times. To run through the whole Harvard sequence, enormous periods would be required, and the existence of stars of so many types shows that, if evolution is a fact, some stars must have done this. The close approach to equipartition of energy indicates that the Universe has probably been in existence long enough for the assumed developments to have taken place.

With regard to the correlation between absolute magnitude and colour, we cannot say very much : it is quite possibly connected with the difference in temperature between giants and dwarfs of the same spectral type. Apart from the effect of strong absorption in the atmosphere of a star, temperature

and colour must change together : the distribution of energy in the spectrum is an index of both. The correlation means simply that when, say, a *G* giant star reaches the *G* dwarf stage, its colour has not progressed so far on the return journey as the spectrum has. We cannot say why this should be so until we know the relation between the atmosphere of a star and the origin of the continuous spectrum.

Finally, we must deal with the strong points of the older theory, and see how they appear in the light of the new idea. Leaving the problem of galactic concentration until the last section, two phenomena remain—the association of the *B* stars with the diffuse nebulæ, and the numbers of visual and spectroscopic binaries of the different spectral types.

The relation between the nebulæ and the *B* stars also must be postponed. It will be appreciated better after we have considered the properties of nebulæ. At this point we will merely say that the occurrence together in space of these two types of body does not necessarily mean that one is being formed out of the other. An explanation has recently been proposed which is even more plausible than the one offered by the former theory of evolution. With regard to the binary stars, however, Russell puts forward the following explanation. Although, owing to their greater brightness, the majority of naked eye stars are giants, yet the dwarfs really outnumber the giants in space. If we confine our attention to the nearer stars, of which our telescopes reveal to us the great majority, irrespective of luminosity, we shall have a preponderance of dwarfs. Now the condition that a star shall be a visual binary is that the angular separation of its components shall be large enough for them to be distinguished ; and this condition will be satisfied generally only among the nearer stars. Hence most of the *visual* binaries are dwarfs, and among the dwarfs Russell's order of development agrees with that formerly adopted.

On the question of the spectroscopic binaries, on the other hand, Russell points out that, as a number of contracting masses of gas grow denser, more and more of them will split up into components. Thus, not the earliest stars, but those well advanced, should contain the greatest number of binaries with small separation. Moreover, the pairs latest formed will have the shortest periods, and will therefore be more liable to detection by the spectroscope. "A large percentage of spectroscopic binaries," says Russell, "especially

of short period, is therefore direct evidence of a fairly advanced state of evolution, and the occurrence of this condition among the stars of classes *B* and *A* supports—indeed, almost by itself compels—the view that they are far removed from a primitive condition. Most of the stars which have been investigated for radial velocity are giants, and the absence of spectroscopic binaries of short period among the redder ones is in agreement with the view that they are in earlier stages of evolution."

Such, then, is Russell's theory. It reduces the many and diverse characteristics of the stars to three. Russell says, in effect: "Give me the mass, density and temperature of a star, and I will tell you its past and future history and its present state." From the mass he will derive the velocity, and estimate how high in the Harvard sequence the star will climb. Add to that the density, and he will deduce the diameter, and discover whether the star is rising or falling in temperature. Knowing also the actual temperature, he will say to what spectral type the star belongs, what is its colour, and what is its absolute magnitude.

There is much in this far-reaching generalisation to give rise to reflection. Taking it as it stands, why do we call it a theory of evolution? We do not seek, and we do not know, the age of a star. The fundamental data—mass, density, temperature—these are existing things, and except for the prophecy of the past and future history of the star—which can never be proved or disproved, and can have only a chimney-corner interest for us—the value of the theory lies in the way in which it summarises other existing things. Evolution appears to be what the chemist calls a "catalytic agent"—an idea introduced for the purpose of deriving certain results, and then removed unchanged.

This, however, is not a fair statement of the case. Evolution, it is true, does not, to any appreciable extent, appear explicitly in the final result, but implicitly it permeates the whole structure of the theory. The validity of the deductions from the fundamental data rests entirely on the assumption that the stars change continuously from lower to higher density. Without that assumption, we cannot go a single step beyond the isolated correlations revealed by experiment. We put the idea of evolution into our observations, but we do not take it away unchanged. It is absorbed, and perhaps

disappears, but it converts a chaotic mass of facts into an ordered and intelligible system, and it is this order and intelligibility that enable us to deduce the whole condition of a star from a few of its qualities.

We might, indeed, go further, and say that, in placing density among the basic qualities of a star, we are effectively placing age there. For if density increases continuously with time, it may be regarded as a measure of time, albeit with an arbitrary starting-point and an arbitrary scale of intervals. It is as good a measure as any other, for that matter. We cannot measure the age of a star in years: we have no means of knowing how long it has lived, and if we had, the very magnitude of the period would baffle our imagination. Before the word "year" had any meaning, the stars in their courses moved and shone very much as they do now. In the tale of cosmic history, our earthly æons are but moments.

Looking at the theory from another point of view, it appears to herald a return to the old mechanistic hypothesis, which sought to explain the Universe in terms of matter and motion. Mass, density, temperature—three dynamical quantities; and from these we can prophesy the spectrum, the output of light, the quality of that light, and all the other characteristics of a star. But it must be noticed that we have not *explained* these characteristics: we have only shown, on the ground of experience, that they can be predicted from a knowledge of what we have called the fundamental ones. Why, for example, should a certain temperature in the atmosphere of a star result in the spectrum we call type *A*? On that question we have not touched. We shall do so in some measure in a later chapter, and we shall find that, for the explanation, we must go deeper than matter and motion, into the structure of matter itself, where the processes that take place baffle as strikingly as they enlighten us.

Or again, what of electricity and magnetism, those mysterious entities which have become the foundation stones of terrestrial physics? Have they no part in the large scale phenomena of the Universe? We are, perhaps, only beginning to understand their influence in the stars and in space. It may, and probably will, be found ultimately that they are the hidden roots of the processes which blossom forth into the obvious phenomena that we measure and record. The Earth has long been recognised as a magnet: we know now

that the Sun is a magnet also, and if the Sun, probably the other stars as well. Not only is there a gravitational connection between the Sun and the Earth; there is believed to be also an electrical connection—a passage of electrified particles, which is closely associated with sunspots, the aurora borealis, and the magnetism of the Earth and the Sun. We can hardly doubt that, if this is so, other stars discharge these electrified particles—actual particles of electricity, in fact. What part do they play in the great cosmic drama?

We must recognise that Russell's theory is not, and does not profess to be, an explanation of things. It is a concise statement of the outward and visible results of what we believe to be the past, present, and future activities of Nature in our stellar universe. The deeper questions—why the course of Nature is what it is; what are the underlying causes of phenomena; what is the nature of nebulæ, and how they are related to the stars; what fundamental influences are at work in the interiors and atmospheres of stars, in particular those belonging to the rarer spectral types—these and other questions must form the subjects of our remaining sections.

In the meantime, Russell's theory gives us a framework into which to fit the miscellaneous facts we have learnt. It shows us the Universe as a connected system, and points to fresh paths towards the answers to the questions that remain. On the one hand, it summarises present knowledge; on the other, it suggests new problems and new solutions to the old ones. It does not account for everything—"I would be rather afraid of it if it did," says Russell—but there are no known facts which are fatally at variance with it.

It may seem presumptuous to generalise for the Universe from the very limited observations we are able to make. Can we, with any semblance of probability at all, say how the stars develop, throughout space and time, when our knowledge is restricted to, at the very most, one two-thousandth of one per cent. of them? I suppose every worker at Astronomy at times has the feeling that the theories and generalisations that grow up and direct our energies may, after all, be illusions; that, in the narrow experience of "God's instant men call years," there is no platform from which to scan the leisure of eternity. It may be so, but there is nothing to be gained by the admission. It is only by following a connected line of thought that progress is possible,

and if we are to rise to greater knowledge, it must be in the arms or on the bodies—it little matters which—of theories which, in their infancy, may need expert nursing to keep them alive. Whatever may be said of Russell's theory as a statement of absolute truth, it is true to the facts we know. If it is destined, like the stars it contemplates, to reach its zenith and then slowly pass out of sight, there is no sign as yet that the zenith is near at hand. Russell's theory is, in every sense of the phrase, a giant theory.

In summarising and co-ordinating the various stellar characteristics, we have made one important omission, which the reader has doubtless observed: we have left parallax, or distance, entirely out of account. The reason is that, with our present knowledge, we cannot satisfactorily bring it into any scheme of stellar evolution: it has no known correlation with age. It relates itself more particularly to a totally different line of thought—the distribution of matter in space rather than in time; in other words, the structure of the Universe. This subject, in its completest form, will be the theme of our last section. Some preliminary account, however, is advisable before we consider the special features of the stars and nebulæ, and the following chapter has, accordingly, been devoted to this purpose.

CHAPTER XIII

THE DISTRIBUTION OF THE STARS IN SPACE

The consideration of stellar distances raises two questions in our minds: first, does the Universe extend infinitely in space, without a sensible thinning out of stars, or is it more or less definitely limited? second, if it is limited, are the limits equally distant in all directions? These questions we are able to answer with a fair amount of precision.

If, however, our knowledge of actual stellar distances were the only means we had of approaching these questions, it would be hopeless to attempt to answer them. Apart from every other difficulty, it is obvious that the Universe would necessarily appear to be finite in extent, because there is a limiting distance beyond which no star is bright enough to be detected. If the Universe extends beyond this distance, then, whether it comes to an end farther out or extends to infinity, our observations will be precisely the same. Clearly, therefore, we must look for evidence of a more general kind than measures of individual stellar parallax will afford.

It should be mentioned, perhaps, that, according to modern ideas, space itself might not be infinite. There are those who think that, if one could set out to travel through space in a given direction, and could continue in the same direction for an unlimited period of time, he would not continually get farther away from his starting point, but would at last retrace the old regions, having put a girdle round space. If that be so, then the idea of an infinite universe is, of course, untenable. But this introduces a problem beyond our present scope. For our purpose, an "infinite" universe means one co-extensive with space, whatever space might be. The practical problem is to determine whether, as we travel farther and farther from the Earth, the number of stars in any convenient unit of volume remains sensibly constant, or gradually decreases until at last it becomes negligibly small.

One fact suggests immediately that there must be a thinning out of stars at great distances, and that is the evidence of the total amount of starlight that reaches us. It is easily seen that if the stars were uniformly distributed throughout an infinite volume, then, if there were no sensible absorption, their light would appear infinitely bright. For consider a sphere, of radius r units, with the Earth at the centre. Let I be the average brightness of a star, and suppose there are n stars per unit of volume. Then, the total number of stars in the sphere is $\frac{4}{3}\pi r^3 n$. Now it can be shown that the light received from these stars at the centre of the sphere is the same as if they were all brought to a uniform distance of $\frac{r}{\sqrt{3}}$. This light would therefore be measured by the quantity $\frac{4}{3}\pi r^3 n \frac{I}{\left(\frac{r}{\sqrt{3}}\right)^2} = 4\pi n I r$; from which it appears that the larger r becomes, the greater is the amount of light received. If, therefore, we suppose r to become infinite, the total amount of starlight becomes infinite also. This is true, whatever values n and I might have; *i.e.* however thinly the stars might be distributed provided the distribution is uniform, and however small might be the average brightness of a star.

This line of reasoning is open to the objection that most of the light might be absorbed in its journey through space, so that the amount we experience gives no evidence on the question. We can, however, apply the argument in a slightly different form. Its essential feature is that, as the sphere grows, the number of stars included increases as the cube, while the light received from them decreases only as the square of the radius. If, now, the stars emit anything other than light, which obeys the same inverse square law, then that something will answer our purpose just as well as light. Now the stars are believed to exert gravitational attraction; *i.e.* they emit gravitational lines of force, which satisfy our conditions absolutely. If there were an infinite, uniform distribution of stars, the gravitational lines of force reaching the Earth would be infinite in number. But they are not. Hence we conclude again that the stars are not spread uniformly throughout infinite space.

But the matter is not yet quite beyond doubt. Even if we grant the assumption that matter everywhere obeys the Newtonian law of gravitation, we are still faced with the

possibility that gravitational lines of force might be absorbed in space. This possibility was not admitted a few years ago, but lately, Majorana has brought forward experimental evidence which he claims establishes the fact of such an absorption. Whether this be so or not is still an open question, but the possibility cannot be ignored. As a rigid proof of the limited extent of the Universe, therefore, this argument falls over the same obstacle as did the last. Moreover, even if there be no absorption of light or of gravitational lines of force, the facts can be explained equally well by assuming either a Universe coextensive with a finite space, or a limited Universe in any kind of space at all. We must seek still

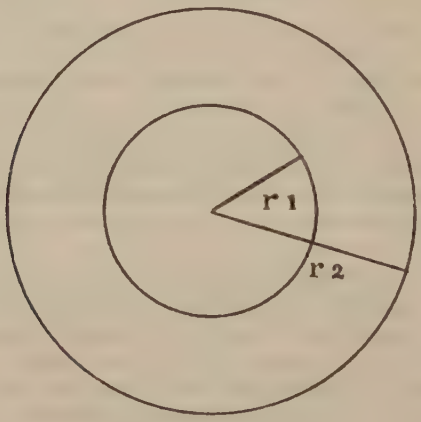


Fig. 12.

further evidence before we can give satisfactory answers to our questions.

The type of argument we have just been using has the advantages and disadvantages of all generalisations. It is direct, simple, and comprehensive, but it gives no knowledge of details. It is, doubtless, this fact, as well as the unsatisfactory character of the conclusions arrived at, that has led astronomers to approach the problem in a different way. We want to know, not only whether the Universe fades out into a surrounding emptiness, but, if so, how gradually the fading out takes place, and what general shape the Universe may be said to have. This information, so far as we know,

can be obtained only in one way—by counting the number of stars of each apparent magnitude.

In Chap. XI. we pointed out that if stars of all magnitudes were distributed uniformly in space, the distant stars perceptible to us would necessarily contain a larger proportion of intrinsically bright stars than would the nearer ones. From the point of view we were then adopting, this was a fact to be deplored, because it left us in doubt whether the distant stars actually were brighter than the nearer ones, or only appeared to be so. We are now to harness our difficulties and direct them to serve our purposes. We shall show that, supposing there is a uniform distribution of stars of all magnitudes, the ratio of the number of stars brighter than one apparent magnitude to the number brighter than the next apparent magnitude must be a constant quantity; and further, that any departure from this constant, found by actual counts of stars, must be attributed to non-uniformity of distribution—supposing again that there is no absorption of light in space. This assumption is evidently very persistent: we shall examine it closely before drawing any conclusions from the following considerations.

Suppose, then, that stars of all magnitudes are distributed uniformly throughout space. We can, in these circumstances, suppose all the stars to have the same intrinsic luminosity, I—the average luminosity of the actual stars. Now, at a distance r_1 (Fig. 12), a star of this brightness would have an apparent brightness, $\frac{I}{r_1^2}$. Suppose this corresponds to apparent magnitude, m. All stars within a sphere of radius r_1 will then appear to be of apparent magnitude m, or brighter, and all stars outside the sphere will appear fainter. Consequently, the total number of stars in the sky, of apparent magnitude m or brighter, will be $\frac{4}{3}\pi r_1^3 n$, where n is the number of stars per unit volume. Now, consider a larger concentric sphere, of radius r_2, such that the apparent brightness, $\frac{I}{r_2^2}$, corresponds to apparent magnitude, $m+1$. Then, similarly, the number of stars in the sky of apparent magnitude $m+1$ or brighter will be $\frac{4}{3}\pi r_2^3 n$. The ratio, therefore, of the number of stars down to apparent magnitude $m+1$, to the number down to apparent magnitude m, is $\frac{r_2^3}{r_1^3}$. But now,

from the definition of magnitude, we know that $\frac{I}{{r_1}^2}$ must be 2.512 times $\frac{I}{{r_2}^2}$, so that $\frac{{r_2}^2}{{r_1}^2} = 2.512$. Consequently, the ratio just given becomes $(2.512)^{3/2} = 3.98$.

It will be noticed that this is true, whatever value m may have. If stars of all magnitudes are distributed uniformly throughout space, then the total number of stars brighter than any one apparent magnitude must be 3.98 times the total number brighter than the next lower apparent magnitude. If, as we approach the fainter stars, the ratio increases beyond 3.98, then the Universe must be more densely populated in distant regions: if the ratio decreases below this number then the stars must be thinning out. Let us look, then, at the results of actual counts of stars.

Inquiries of this kind have been made by several investigators, including Kapteyn, Van Rhijn, Seares, and Chapman and Melotte. After considerable discussion, a substantial agreement has been reached, so that there is now little doubt as to the main facts. The data considered cover a large number of different regions of the sky, which are taken to be sufficiently representative to give reliable estimates for the whole heavens. The results confirm the conclusion arrived at long ago by Sir William Herschel, that our universe of stars has more or less definite boundaries, and a fairly symmetrical shape.

In the following table, the figures are given for the whole sky, according to the conclusions of Van Rhijn.[1] The magnitudes given in the first column are estimated on the visual scale. It will be obvious, of course, that a ratio of 1 in the 3rd column would indicate that the actual limit of the Universe had been reached, but higher values of the ratio are not necessarily inconsistent with the same conclusion, for the figures for the higher magnitudes may be enriched by the intrinsically faint and near stars, as well as by the bright stars in the outlying regions of space.

[1]The figures beyond the 16th magnitude are extrapolated, but are in general agreement with Seares's investigations at Mount Wilson.

TABLE VI

Total Number of Stars brighter than each Unit of Visual Magnitude.

Apparent Magnitude *m*	Total Number of Stars *N*	Ratio	Apparent Magnitude *m*	Total Number of Stars *N*	Ratio
0	3		10	380,200	
		3.7			2.7
1	11		11	1,026,000	
		3.5			2.5
2	39		12	2,588,000	
		3.4			2.3
3	133		13	5,894,000	
		3.4			2.2
4	446		14	13,120,000	
		3.3			2.1
5	1,466		15	27,540,000	
		3.2			2.1
6	4,732		16	57,150,000	
		3.2			1.9
7	15,000		17	107,200,000	
		3.1			1.8
8	46,240		18	197,200,000	
		3.0			1.7
9	139,300		19	335,000,000	
		2.7			1.6
10	380,200		20	530,900,000	

It will be noticed that the theoretical ratio, 3.98, corresponding to uniform distribution of stars, is almost attained for the brightest stars, but that, as successively fainter stars are considered, the ratio gets less and less. The obvious meaning of this, if we neglect the absorption of light in space, is that the stars must thin out continuously at great distances, for, the fainter the stars dealt with appear to be, the greater will be the volume of space in which they are distributed. The Universe thus gives every indication of coming to a limit, and we seem to be situated in its densest portion. In an inquiry of this kind, of course, only the general outline of the Universe is revealed. Star clusters, and other possible

aggregates of stars, if they belong to our own stellar system, will form conspicuous irregularities in it, and must be the subject of separate investigation when we have settled the main features.

A limited Universe must, of course, have a shape, and we must next inquire what that shape may be. On looking at the sky, no one can fail to notice that the stars appear much more crowded in the direction of the Milky Way than they do in the perpendicular directions. From the mass of faint stars which the Milky Way shows, we are led to conjecture that we might be looking through a greater depth of stars in that direction than in any other. The difference is so marked that we can hardly avoid this conclusion—though really it is a very hazardous one, considering that the naked eye stars do not number more than four or five thousand (see Table VI). Photographs, however, show the concentration of stars towards the Milky Way even more strongly than it appears to the naked eye. Consequently, researches into the shape of the Universe have always proceeded along the line of counting the numbers of stars in belts at different angular distances from the Milky Way. The central plane of the Milky Way is almost a great circle in the sky. It is convenient, therefore, to use it as a circle of reference for defining the position of a celestial object. *Galactic longitude* and *galactic latitude* are, in fact, the co-ordinates generally used in discussions of the Universe as a whole. The former is measured along the Milky Way, from the point of its intersection with the equator in the constellation Aquila (right ascension, 18 h. 40 m.): galactic latitude is the angular distance north or south of the central galactic circle.

Our inquiry into the shape of the Universe accordingly takes this form. Admitting, tentatively, that the Milky Way defines the plane in which the stars are most thickly crowded, what is the relation of this plane to the whole stellar system? Is it a plane of symmetry, or is its significance exhausted when we have recognised how populous it is? Further, what is the character of the crowding of stars in the Galaxy? Is it that the stars are packed equally densely in all directions, but that the Universe extends much farther in the direction of the Milky Way; or is it that the Universe is globular in shape, and that the number of stars per unit volume is greater in the galactic directions? These are evidently highly pertinent questions, which we must attempt to answer.

In the first place, we may say at once that counts of

stars in belts of equal galactic latitude, north and south, show that the Milky Way is actually a plane of symmetry for the stellar system. It must be added, however, that, for this to be strictly true, the fundamental galactic circle must be regarded, not as a great circle in the sky, but as a small circle a little south of the great circle parallel to it. All that this means, of course, is that we must regard ourselves as being situated a little north of the central plane of the Universe. Accepting this position, we may view the stellar system, so far as counts of stars reveal it, as a structure symmetrical about the galactic circle.

The precise character of the symmetry raises a more difficult question, to answer which we need further data. Table VII contains the substance of Table VI, analysed so as to show how the counts vary with galactic latitude. The numbers given are the average numbers of stars per square degree, and the galactic latitude represents, in each column except the last, the mean value for a belt 10° in thickness. The last column refers to a galactic polar cap of radius 20°. In each column, the mean value for regions north and south of the Galaxy is given. The figures in small type are the ratios of the numbers for successive magnitudes.

TABLE VII

Apparent Magnitude *m.*	Average Number of Stars per Square Degree in Regions of Mean Galactic Latitude							
	5°	15°	25°	35°	45°	55°	65°	80°
8.5	3.3	2.4	1.9	1.5	1.4	1.3	1.3	1.2
	3.2	3.0	2.9	2.9	2.8	2.8	2.7	2.7
9.5	10.4	7.3	5.5	4.4	3.9	3.7	3.5	3.4
	2.8	2.7	2.6	2.5	2.5	2.4	2.4	2.3
10.5	29	20	14	11	10	9	8	8
	2.8	2.7	2.6	2.5	2.4	2.4	2.3	2·2
11.5	81	53	36	28	23	21	19	17
	2.6	2.5	2.4	2.3	2.2	2.1	2.1	2.0
12.5	209	130	86	63	51	44	39	35
	2.4	2.3	2.2	2.1	2.1	2.0	1.9	1.8
13.5	507	301	192	135	105	88	75	64
	2.2	2.2	2.1	2.0	1.9	1.8	1.8	1.8
14.5	1138	676	398	267	200	160	132	112
	2.2	2.2	2.0	1.9	1.8	1.8	1.7	1.7
15.5	2483	1479	800	514	369	282	229	195
	2.2	2.1	2.0	1.8	1.8	1.8	1.7	1.7
16.5	5495	3162	1585	933	661	501	398	331
	2.2	2.1	2.0	1.8	1.7	1.7	1.7	1.7
17.5	12020	6607	3090	1660	1148	871	692	550

It appears from this table that the number of stars per square degree grows as we approach the Milky Way, for all magnitudes brighter than 8.5. This was to be expected for the stars as a whole, but the fact that it holds for the relatively brighter as well as the fainter stars, tells us something definite about the structure of the Universe. It means that, whether or not the Universe stretches farther in the galactic plane than it does in the perpendicular directions, the stars must be more congested in that plane, even at comparatively short distances from us. For, in dealing with stars whose apparent magnitudes are as low as 8.5, we can hardly suppose that we touch the limits of the Universe in any direction. The increase of star density[1] in the galactic plane must, in part at least, be attributed to a greater number of stars per unit volume in that plane, compared with the number in an equally distant unit of volume in the higher galactic latitudes.

From the existence of a " galactic concentration " for the lower magnitudes, then, we conclude that the stars begin to thin out more rapidly towards the galactic poles, even at relatively short distances, than they do in the galactic plane. There is, of course, a thinning out in all directions, for the theoretical ratio of 3.98 is never reached, but the rate of thinning out is by no means uniform. But now, there is another conclusion to be drawn from Table VII. It will be noticed that, as we include fainter and fainter stars in our considerations, the relative numbers in the galactic plane grow greater and greater. This is best expressed by what is known technically as the *galactic concentration, i.e.* the ratio of the numbers of stars in the second and last columns of Table VII. From the table below, which is derived from the figures in Table VII., we see that the galactic concentration increases continuously as the fainter stars are included in the counts :—

[1] By " star density " we here mean, of course, the number of stars per square degree. It is not to be confused with the density of an individual star, which was discussed in Chap. X.

TABLE VIII

Apparent Magnitude.	Galactic Concentration.
8.5	2.6
9.5	3.1
10.5	3.7
11.5	4.7
12.5	6.1
13.5	8.0
14.5	10.1
15.5	12.7
16.5	16.6
17.5	21.9

Now what conclusion can be drawn from this? There can be little doubt that it means that we are dealing with a universe that is considerably flattened in the galactic plane, and that probably our telescopes can pierce towards the galactic poles into regions where the stars are almost negligibly few. It is not imagined that there is a definite boundary outside which there are no stars at all. We think rather of a gradual thinning out of stars, in such a way that surfaces drawn through regions of equal star density would be something like the outline of a circular biscuit. The reason why a universe of this kind would give the data of Tables VII and VIII may be seen in the following way.

In Fig. 13, let the dotted curve represent a section, by a plane through the galactic poles, of a somewhat arbitrary boundary of the Universe such that the stars outside it are very few. Suppose the most luminous star in existence to have a brightness, I, and let the radii of the circles in the figure represent the distances at which it would have apparent magnitudes, m, $m+1$, $m+2$, etc. Then, all stars brighter than apparent magnitude m will be included within the circle m: those brighter than apparent magnitude $m+1$ will be included within the circle $m+1$, and so on. Suppose, for simplicity, that we are at the centre of the system, and

consider the number of stars seen on equal areas of the sky in the directions of the galactic plane and the galactic poles.

Suppose first that we count only the stars brighter than apparent magnitude m. These will all be contained in the equal volumes, OAB and $OA'B'$, and the excess of the number seen in the galactic plane will be due entirely to the fact that, for equal distances, the stars are more densely packed in that plane than in other directions. This will be true so long as we confine ourselves to stars brighter than apparent magnitude $m+n$: for that and lower magnitudes, the excess of the stars in the volume $OC'D'$ over those in the volume OCD will be due to the greater density of packing of the stars.

But now suppose we include stars brighter than apparent

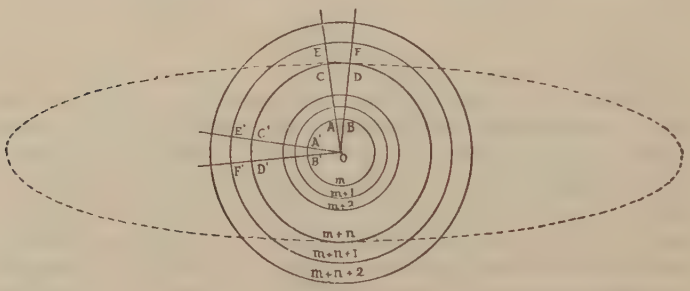


Fig. 13.

magnitude $m+n+1$. Then we add to the stars in the galactic plane a large number from the volume $C'D'E'F'$, while the number towards the galactic poles will be augmented only by the negligible number in the volume $CDEF$, outside our arbitrary boundary. (We can leave out of account the near, intrinsically faint stars that will come in in both directions). Consequently, there will be a large increase in galactic concentration as we go from apparent magnitude $m+n$ to apparent magnitude $m+n+1$—an increase which will be repeated for each successive step of one magnitude. This, of course, is just what Table VIII shows. The increase of galactic concentration is slow at first, when it arises only from the more rapid thinning out of stars in the polar directions. But for the higher magnitudes it goes up immensely,

and continues to do so to the end of the table. We conclude, then, that, towards the galactic poles, we can detect stars which are effectively at the limits of the sidereal universe.

Summing up, then, we may say that, considering the Universe in its most general aspect, we belong to a system which is not coextensive with space, and is not without form. It is a system having a definite shape, and showing a very marked symmetry of construction. Though it has no assignable limits, yet it fades away ever as we travel outwards—a sort of "infinite isolation," to borrow an expressive phrase from Thackeray. It is a system which we can consider alone, apart from whatever lies beyond.

The details of this system we must, in the main, postpone until we come to the last section. It is necessary, however, to deal at once with the question of galactic concentration of stars of the separate spectral types, the chief facts concerning which were stated in the last chapter. We saw there that with advancing spectral type the galactic concentration becomes less and less. This can, in general, be explained in terms of the average absolute brightness of stars of the various types. Taking giants and dwarfs together, it is clear that the average absolute brightness decreases from *B* to *M*. The spherical region of space within which we can detect stars therefore shrinks as we go from the beginning to the end of the spectral sequence, and the galactic concentration must consequently decrease for the reason we have already explained. The decrease, however, is found not to be steady. There is a precipitous fall when we pass from the earliest *A*-type to the later *A*-type stars, and this seems to throw some light on a fact to which we have already drawn attention; namely, the apparent scarcity of giants of *F* and early *A* type. Apparently the giants of these types are abnormally faint compared with the giants of other types. Shapley considers that the average *A*2 star may be more than a magnitude fainter absolutely than the average *A*o star. This is a very interesting and curious result, which has not yet received a satisfactory explanation.

With regard to the relative numbers of stars of the different spectral types, we may give the following table, due to Shapley, which shows, for all longitudes in the central plane of the Milky Way, the average number of stars in a region of one million cubic parsecs.

TABLE IX

*B*0 —*B*5	4·4
*B*8 —*A*3	250
*A*5 —*F*2	680
*F*5 —*G*0 (dwarfs only)	7600
*G*5 —*K*2 (giants only)	160
*K*5 —*Mc* (giants only)	22

The enormous preponderance of *F*-*G* type dwarfs (stars like the Sun) is particularly noticeable. It may mean that a star remains for a relatively very long time in the state corresponding to this type, but we have no certain knowledge on the matter.

The conclusions we have reached in this chapter cannot be accepted without the greatest amount of circumspection. Obviously, they depend on the accuracy of the star counts and of the measurements of apparent magnitude. The latter measurements, in their turn, presuppose a reliable scale of magnitudes, in which each unit corresponds to exactly 2.512 times the brightness of the next. Until recently, it could not be said that the reliability of our data was sufficient to give much weight to deductions so far-reaching as those we have here presented. But to-day, thanks largely to the labours of Seares and others, we are more fortunately situated. On these grounds there is no longer room for doubt as to the general character of the structure of the Universe. The figures may be altered somewhat in the future, especially for the fainter magnitudes, but their general meaning can hardly be changed.

The most serious objection to our conclusions, however, comes from the possibility of absorption of light in space. How serious this objection is may be gathered from the following remark of Kapteyn's : " Unless we succeed in determining the absorption constant within at least something like a thousandth of a magnitude, no really definitive results can be obtained for the arrangement of the stars in space." It is necessary, therefore, to consider this matter with great care.

Absorption of light may be of two kinds—absorption by isolated opaque bodies, or general absorption by dust or fog.

We know that the first kind exists. We have periodic eclipses of bright stars by dark ones, and also there is every reason to believe that there are dark nebulæ of considerable extent. Turner, in fact, has suggested, from the evidence of photographs of different parts of the sky, that there exists a "spiral of obscuration," whose outline he has indicated, and which does not lie in the galactic plane. However this may be, there is no doubt that opaque bodies exist and hide from us the light of the stars.

But absorption of this kind is not of much consequence from our present point of view. We do not attempt to count all the stars in the sky, and the regions we select as typical are in all probability not troubled by these dark bodies. Of infinitely more consequence is the possible existence of cosmic dust, which does not obscure the stars entirely but diminishes their brightness and so introduces a new factor into the relations between their apparent magnitudes. And dust is precisely the form in which a given quantity of matter can have the greatest effect in obscuring light, for the greater the number of particles into which a body is broken, the greater is the total surface which it exposes. Macmillan has calculated that, supposing the stars actually to be distributed uniformly to infinity, the existence of one particle of dust, one thousandth of an inch in diameter, in every 560 cubic miles of space, would be sufficient to reduce the star ratios for successive magnitudes to the figures we have given and would account for all the conclusions we have drawn. Expressed in this way, it seems hopeless ever to be able to place confidence in investigations made along the lines we have chosen; but there is another aspect of the matter. One such particle in 560 cubic miles seems an inconceivably small quantity, but, so vast is interstellar space, that in the 40 cubic parsecs which would be the Sun's share if space were partitioned among the stars, such a distribution of dust would provide nearly seven times as much matter as there is in the Sun itself. If it is hard to believe that so sparse a scattering of dust can be proved non-existent, it is equally hard to believe that "empty space" contains many times as much matter as the whole of the stars. We must attack the problem in some other way.

The effect of dust on light would probably be to "scatter" rather than to absorb it. The phenomenon of scattering by small particles is well known, and the principles by which it

takes place have been closely studied. Scattering is much more effective with shorter waves than with longer ones. Thus, the blue colour of the sky by day is attributed to the shorter waves in the solar light, which are scattered by particles in the Earth's atmosphere. At sunset, when we are looking directly towards the Sun, we see a red disc because the blue light has been scattered before reaching us. If, now, space contains enough dust to lower our measures of the apparent brightness of the stars, the distant stars should appear redder than the near ones of the same temperature and spectral type, for they will be robbed chiefly of their blue light. Along these lines, then, the question of light absorption or scattering has been approached.

The most conclusive investigation of this kind has been made by Shapley. By several arguments he has shown that the great star cluster in *Hercules* (Plate XLIII) is very distant —much more distant than, a few years ago, any known object, except, perhaps, the spiral nebulæ, was believed to be. He has measured the colour indices of stars in this cluster, of known spectral type, and he has found that there is no appreciable difference between them and the values for our nearer neighbours of the same types. He finds a number of negative colour indices, corresponding to classes *b*0 to *b*9—which would hardly be possible if there were any detectable amount of cosmic dust in the intervening space. He has obtained, also, confirmatory evidence from other regions in space, both in the galactic plane and away from it. Consequently, it is now generally conceded that, if there is any absorption or scattering of light in space, it is too small in amount to vitiate the conclusions arrived at with regard to the structure of the Universe. The evidence is not confined to Shapley's observations. Several investigators have drawn similar conclusions from the same and other kinds of argument, and although attempts have been made to express a supposed slight amount of scattering by a mathematical formula, there is now a general agreement that, whether or not scattering exists, it is negligible in amount.

After the most thorough examination, therefore, we have no alternative to the conclusion that the Universe has the shape and limitations we have indicated. But we must insist that we have made only a first approximation to the actual structure of our system. We have found its outline:

its details we have ignored. What, for example, is the Milky Way? Is it merely a name for a great thickness of stars, or is there, beyond the stellar system, an encircling ring of matter which is more truly entitled to the name? Again, what are the star clusters and nebulæ? Do they belong to our system, or do they lie outside—perhaps other and similar universes? Moreover, even within our own system there are things we have yet to understand. What is star-streaming? What are moving clusters? How is the vast system which we have brought to light acting in its inward parts? Is it a complex body, like the higher organisms, or is it a collection of smaller universes, modelled after the fashion of the whole, and acting and reacting with one another within the larger cosmos? To these questions we will turn in the last section, and see how far we can give them an answer.

But meanwhile we have another task to perform. We must look into the nature of the star clusters and nebulæ, and see what we know about them in themselves before assigning to them a place in the cosmic scheme. The Milky Way also must claim our attention. Further, though we have spent so many chapters with the stars, we have not yet looked into a single star to see what is going on there. We have seen and classified the results of stellar activities: we must pierce now into the stars themselves—into their atmospheres, where the spectrum lines are born; into their yet more mysterious depths, whence comes their light. And, since the Sun is our nearest star—so near that we can study it piece by piece—we must give it our special care, and learn from it things which the distant orbs of night could perhaps never tell us. This, then, is our work in the next section of our inquiry. Afterwards, we shall see how, with our deeper knowledge, the Universe appears.

Section III

VARIETIES OF COSMIC BODIES

CHAPTER XIV

THE SUN

If one were to inquire which of the heavenly bodies most engages the attention of astrophysicists, the answer, without hesitation, would be, the Sun. And this is not surprising: the Sun has always held the first place in the astronomer's work, and before astronomy was born it could boast its worshippers in almost every tribe of men. It is the ruler of our solar system: on the Sun we depend for our energy, our means of life, even our very lives themselves. Small wonder if the devotion of the infant soul is succeeded by that of the maturer mind.

But, from the purely astrophysical point of view, it is not the supremacy which the Sun claims as the monarch of our solar system that entitles it to the first place in modern investigation. The attention lavished on the Sun is no "echo of fair, ancient flatteries." It is, indeed, no flattery at all—rather the reverse. To the eye of astrophysics, in a fine calmness sweeping the Universe from its centre of gravity, the Sun is as inconspicuous an object as any that comes within its view. It is no star of stars: it is—at least, so we hope and believe—utterly mediocre and undistinguished. From the exalted point of view which we strive against our limitations to adopt, the Sun is a number in a catalogue, a point on a photographic plate.

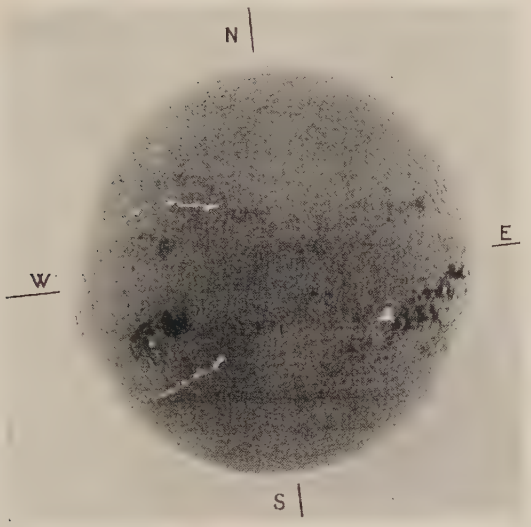
Why is it, then, that the Sun yet holds for us an interest and a fascination beyond those of any other celestial body? It is simply that, because it is so near as to show a finite size, we can examine it in detail, see what it looks like, and submit it, piece by piece, to examination. And not only so, but, turning to account the very mediocrity of the Sun, we claim thereby to discover the constitution of millions of similar stars. The Sun is the great betrayer: it yields, not only its own secrets, but those of its neighbours as well. In drawing this conclusion, we are, of course, making an assumption, though a very reasonable one: we are assuming that the uniformity of Nature is so complete that stars which resemble

one another as a whole will resemble one another also in the character and relations of their separate parts. We cannot prove this, but we have every reason to believe it to be so. In all the qualities we have considered—spectrum, magnitude, temperature, and the rest—the Sun is absolutely typical of the stars of its class. It would be surprising indeed if, in detail, it showed wide divergences from them.

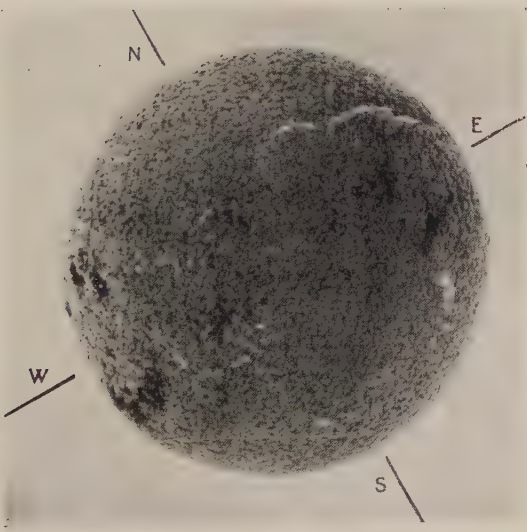
Let us review, for a moment, the character of the knowledge we have been able to gather from the stars in general, so that we can see more clearly what special advantages the study of the Sun is likely to afford. To the eye and the telescope, the stars are but points. The interferometer, however, shows that a few of them, at least, have dimensions, and we are obviously on safe ground in assuming that this is true of them all. The spectroscope tells us something more: it reveals two distinct parts of a star, which give rise respectively to the continuous spectrum and the absorption lines—parts which we are able to call the *interior* and the *atmosphere*, because the character of the spectrum shows us their relative positions. But there our knowledge ends: the analysis of a star can at present be carried no further. The qualities we discussed in the last section characterise either the star as a whole, or one of these two parts as a whole. Parallax, mass, and density characterise the complete star. Velocity—though a hypercritic might say that its radial component is measured for the atmosphere alone—can also be attributed to the complete star. Magnitude, effective temperature, colour, and diameter are functions chiefly of the interior. The spectrum reveals both interior and atmosphere. What parts of the interior or the atmosphere give rise to the various phenomena, or indeed, if these regions have any parts at all, are questions which we ask of the stars in vain.

It is here that the Sun comes to our aid. In this particular star, the interior is no longer merely the hypothetical origin of a continuous spectrum. It is something which we can see and examine—at least on the surface. It is known as the *photosphere*, and appears as the bright disc which we see when we look at the Sun. The atmosphere also is something that we can analyse. It is an envelope of finite thickness, varying in some respects from layer to layer. It is not sufficient to say that a spectrum line originates in the atmosphere: we ask, and discover, from what part of the atmosphere —or *chromosphere*, as it is usually called—the line arises.

PLATE XIX.



(*a*)



(*b*)

Photographs of Solar Atmosphere.

(*a*) In Hydrogen (Hα) light.
(*b*) In Calcium (K_3) light.

To face page 182.

Further, we can investigate the character of the discontinuity between interior and atmosphere. Is there a layer of some kind separating these regions, or do we see straight into the heart of the Sun itself? Finally, we can look for other possible solar appendages, outlying the atmosphere (*i.e.* the place of origin of the absorption lines), which, in other stars, because of their distance, may be hidden from us. These are the kinds of problem which, by a study of the Sun, we may hope to solve.

But before we begin to face them, it is well that we should see clearly how the Sun stands with regard to the general qualities which we have considered for the stars at large. It is essential that we should know this, for otherwise we should not know to what stars our knowledge of the Sun could be extended with the least amount of modification. To begin with the spectrum, then, the Sun is a typical dwarf star of *G* type—so that, according to Russell, it is well on the road to extinction as a luminous body. Its apparent magnitude is -26.72, but this, of course, is of no significance for our purpose. Its absolute magnitude, however, is $+4.8$. This means that, if it were removed to a distance of ten parsecs—where it would still be among the very nearest of the stars—it would appear almost among the faintest naked-eye stars in the firmament—an inconspicuous object in the sky. The Sun's motion in space we have already considered. Its distance from the Earth is about 93 million miles—a quantity which, since it must be known before other stellar distances can be measured, must obviously be determined by special means, the consideration of which is outside our scope. The effective temperature of the Sun we shall consider separately, since we are able to measure it from the heat, as well as the light, which the Sun sends us. In colour, the Sun is true to its type. Its mass, which we took as the unit in the last section, is determined, from its gravitational attraction on the Earth, to be 4.38×10^{30} pounds—332,000 times as great as the mass of the Earth. Its density is 1.4 times that of water, and its diameter is 865,000 miles. The Sun is, therefore, so far as we can judge, in every respect typical of the stars of its own spectral class.

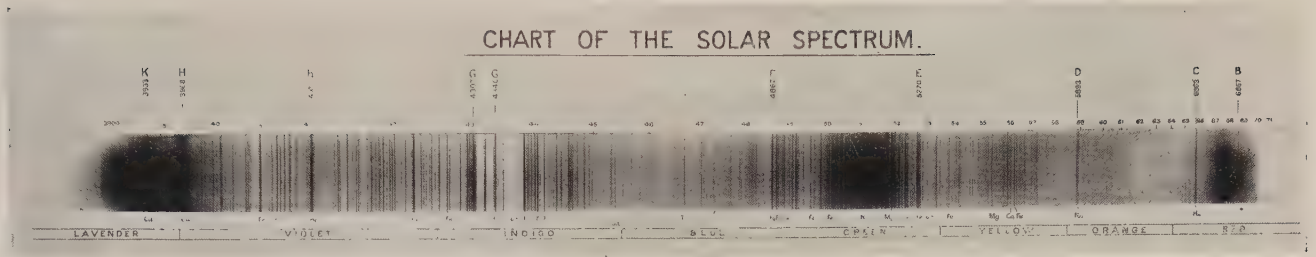
Starting with this knowledge, then, we will look first at the atmosphere, or chromosphere, of the Sun. Our chief source of information is, of course, the absorption lines in the spectrum, and we may remark that, on account of the Sun's great apparent brilliance, we can study these lines under

the best possible conditions. For we can employ the highest dispersion our instruments will give us—a thing impossible with any other star, because the more the light is spread out the weaker it becomes in any part of the spectrum, and to get sufficient light to give a visible spectrum of a star, the dispersion employed must be small. But with the Sun, there is not this difficulty : the solar spectrum, in fact, can be photographed with such perfection (see Plate XXVI.) that, until recently, it was used to give the standard lines by which the wave-lengths of lines produced in the laboratory were measured. It is largely on account of this perfection that the solar spectrum tells us so much more than the spectrum of a more distant star can reveal.

When we come to interpret the observations, however, we must be on our guard. Not every line in the solar spectrum is a native of the Sun : there are intruders from our own atmosphere. The solar light, passing through miles of air before it reaches our instruments, is robbed on the journey of much of its intensity. The most serious loss is the whole region of the ultra-violet spectrum beyond wave-length 2950 —a loss which, from experiments of Fowler and Strutt (now Lord Rayleigh), we attribute mainly to absorption by atmospheric ozone. This absorption is disastrous from more than one intellectual point of view, though, no doubt, it is well for our bodies. But we are concerned now with a more subtle loss—the selective absorption, by our atmosphere, of light of particular wave-lengths, corresponding to the radiation possible, in certain circumstances, to the atmospheric gases. This absorption results in dark lines in the solar spectrum which, without special investigation, it is impossible to distinguish from the true solar lines.

There are two methods, however, by which the distinction can be made. The first method is to compare photographs of the solar spectrum taken respectively with "high" and "low" Sun. When the Sun is high in the heavens, its light falls on our instruments almost vertically, and therefore takes almost the shortest possible path through the atmosphere. A rising or a setting Sun, on the other hand, sends its beams through a much greater thickness of air, and loses the maximum amount of light during the journey. Consequently, the lines in the spectrum which are of terrestrial origin—the *telluric* lines, as they are called—are considerably strengthened in the spectrum of the low Sun, relatively to the

PLATE XX.



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true solar lines. This strengthening is the hall-mark of the telluric lines. The same principle may be applied, with modification of details, to detect lines arising from water vapour in the atmosphere. The humidity of the air varies considerably from day to day, and by comparing solar spectra photographed through moist and dry air, respectively, the criterion we have mentioned again reveals some of the telluric lines.

The second method is less direct, but equally useful. We shall see shortly that the Sun rotates on an axis in such a way that, at any instant, the eastern limb has a large component of velocity towards the Earth while the western limb is moving equally quickly away from it. The spectrum lines originating in these regions should therefore show Doppler displacements in opposite directions—as, in fact, they do. But the lines produced in the Earth's atmosphere, which is practically stationary relative to the spectroscope, can suffer no such displacement. To distinguish telluric lines, therefore, all that has to be done is to take simultaneous comparison photographs of opposite equatorial limbs of the Sun. The lines which occupy identical positions in the two spectra are the telluric lines.

Supposing, then, that we have identified our true solar spectrum, what can we learn from it ? In the first place, we observe that the Sun's atmosphere is made up of more than one layer. Plate XXI shows the H and K lines of ionised calcium as they often appear. Each line is made up of three parts. There is, in the background, a broad, dark line : on this appears a narrower bright line ; and in the centre (or almost the centre) of this a fine dark line is to be seen. These three portions have come to be known as H_1, K_1 ; H_2, K_2 ; H_3, K_3 respectively. Their message is clear. In the lower part of the Sun's atmosphere there is a layer containing ionised calcium (and, of course, other things as well). By absorbing the light of the interior, it gives rise to the broad, dark lines, H_1, K_1. Above this, however, there appears to be another layer, which also contains ionised calcium, and this produces the bright emission lines, H_2, K_2. But not all of their light reaches us. Some is absorbed by a third layer of lower density, and so we get the fine dark lines, H_3, K_3. The differences in width of these three lines may be attributed to the fact that the higher we ascend in the Sun's atmosphere, the

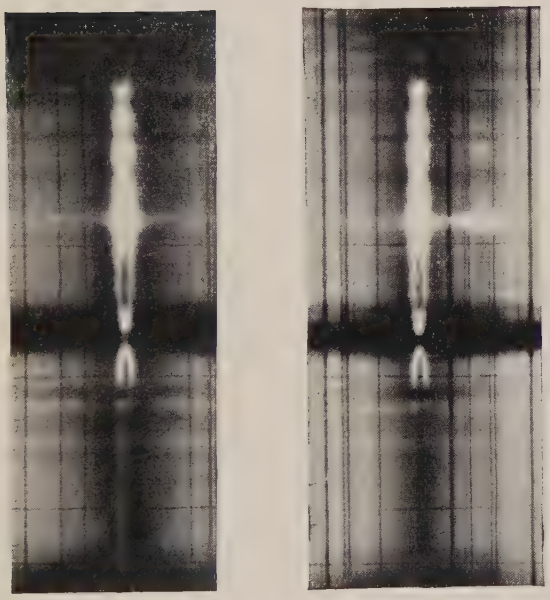
lower the pressure will become, and so the narrower will be the spectrum lines produced.[1] (See p. 37.)

The Sun's atmosphere, then, is not homogeneous: it possesses a structure. It must not be imagined, of course, that this structure is in any way rigid. On the contrary, the atmosphere of the Sun is always in a state of most violent agitation. Portions of it move at times with enormous velocities, as frequent contortions of the spectrum lines testify. The fact which we desire to emphasise is that the lines in the solar spectrum do not necessarily all arise from the whole of the solar atmosphere. Each has its own place of origin, and while the lowest levels may give rise to some, others are produced at the highest points. This is a matter of great importance. We must now carry our investigations further, and try, if possible, to assign to each line the level or levels from which it arises. We say "each line," and not "each element," advisedly, because it by no means follows that if one line of hydrogen, say, is produced in a certain region, all the other hydrogen lines are produced there also. Chemical analysis, when compared with the operations of spectroscopy, is a very coarse process. It traces a mixture of substances down to its elements—its atoms—but it can go no farther. But to the spectroscopist, every *state* of an atom is a distinct entity. A hydrogen atom about to emit the red line is a different thing from a hydrogen atom about to emit the blue line, and each kind of atom is the fitter to survive under its own physical conditions, and therefore in its own regions of the Sun's atmosphere. Of course, some regions may contain both kinds of atom, but the relative numbers—and therefore the amount of their contribution to the complete spectrum—will vary from place to place. It is necessary, then, that each line shall be regarded as speaking only for itself, and not as a representative of all the lines produced by the chemical element from which it arises.

To determine precisely where the various solar absorption lines—the Fraunhofer lines, as they are called, from one of their earliest observers—arise, we must consider them alone, apart from the continuous background of the spectrum. This at first seems absurd—like trying to think of a circle without its circumference—but we must remember that the Fraunhofer

[1] Other lines, besides *H* and *K*, show this effect. *H* and *K*, however, show it most clearly, partly because they are the strongest lines in the solar spectrum.

PLATE XXI.



Triple Reversal of H and K lines in Solar Spectrum.

(The horizontal dark band across the centre of the lines arises from the presence of a sunspot.)

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lines are not absolutely dark (see p. 16); they contain, in fact, a great deal of light, and appear to be dark only by contrast with the brilliant background against which they are seen. The atmosphere of the Sun is capable of absorbing light of certain wave-lengths, only because it can itself emit exactly similar light. It absorbs the intense light of the interior, and emits feebler light of the same wave-lengths which, by contrast, appears as dark lines in the spectrum. If we could blot out the light of the interior, the Fraunhofer lines would show up bright, as an ordinary emission spectrum, which indeed they are. The problem is to eliminate the background, and show them in their true colours.

A solar eclipse at once suggests itself as a favourable opportunity of attacking the problem. As the Moon gradually creeps over the face of the Sun, there comes a time when it just obscures the last remaining edge of the photosphere and leaves the atmosphere at the limb exposed to view. Then, for a brief period, the atmospheric light can be analysed, and bright lines are seen in the positions of the Fraunhofer lines which appeared a moment before. It is evident that, if we could take a succession of photographs of the spectrum while the Moon is passing across the solar atmosphere, we should be able to get the information we are seeking. For, at first, the whole depth of the atmosphere is exposed, and we should get the complete spectrum. The lowest layers are the first to be obscured, so that the lines which would disappear first would be those produced at the bottom of the atmosphere. The earlier a line disappeared, the lower would be its place of origin, and so, by arranging the lines in the order of their defection, we should be arranging them also according to the level at which they are produced.

The difficulty in carrying out this process is that the relatively small thickness of the solar atmosphere subtends so minute an angle at the distance of the Earth, that the Moon covers it in an extremely short time, during which it is possible to get only a few photographs of the spectrum. The bright line spectrum obtained in this way is spoken of, in fact, as the "flash" spectrum, on account of its evanescence. Nevertheless, it is possible to do something along the lines we have suggested. Plate XXII. shows three photographs of the flash spectrum taken at different moments during the passage of the Moon across the Sun's atmosphere at the eclipse of 1898. It is obvious at once that the majority of

the lines arise from the lower levels—the lines, chiefly, of the metals, so characteristic of the *G* type of spectrum. These low levels constitute what is often called the "reversing layer" of the atmosphere. At the higher points, hydrogen and helium are shown, as well as the *H* and *K* lines of ionised calcium. Broadly speaking, these results alone show that there is a general tendency for the heaviest substances to sink to the bottom, while hydrogen and helium, the two lightest elements known, rise to the top. Why ionised calcium, a far heavier substance, should be at the top as well, although there is a complete absence of elements of intermediate weight,

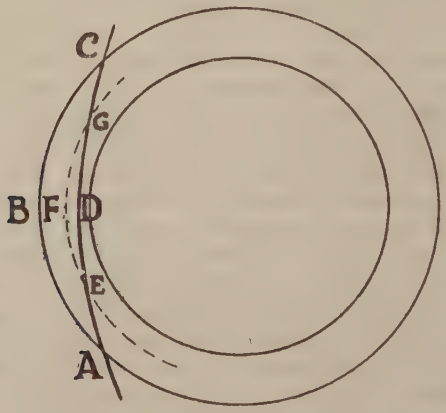
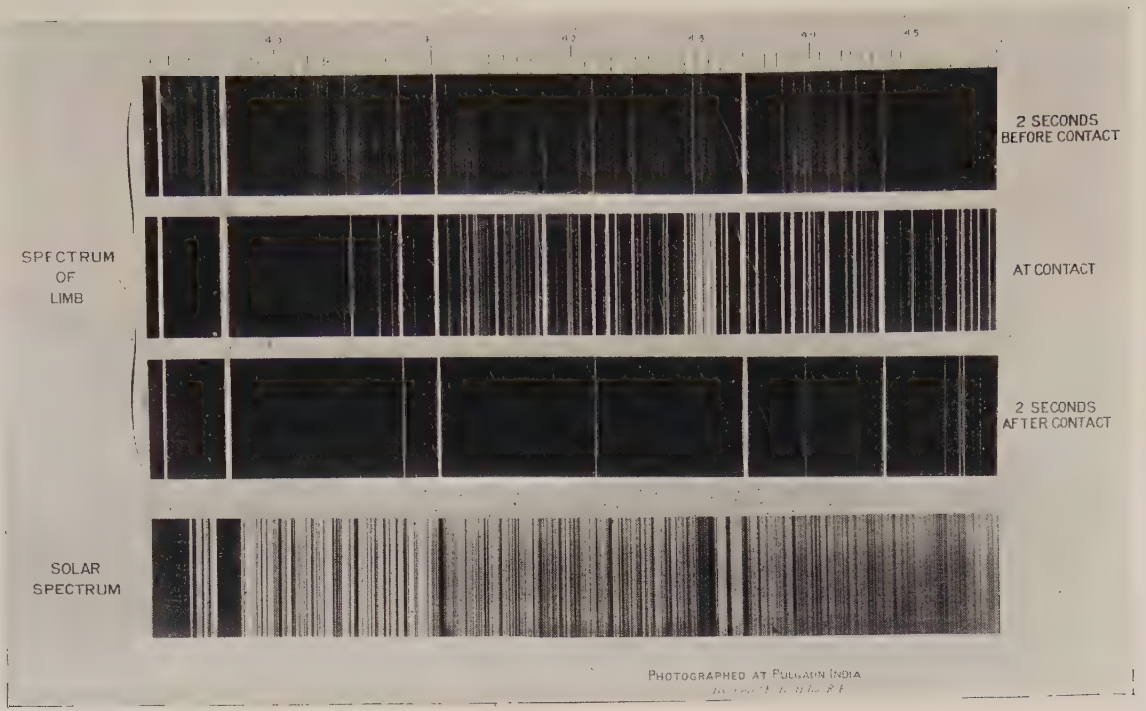


Fig. 14.

has long been a puzzle, to which we have only recently found an answer. But more of this anon.

A more satisfactory way of obtaining the flash spectrum is to use a spectroscope having neither slit nor collimator, and so to obtain each line as a nearly circular arc—an actual image of the portion of the exposed atmosphere which gives rise to it. In these circumstances, it will readily be seen that the length of the arc corresponding to each line is a measure of the height in the atmosphere reached by the atoms producing the line. For, in Fig. 14 (not drawn to scale), let the two complete concentric circles represent the upper and lower limits of the Sun's atmosphere, and let the arc of larger radius represent the edge of the Moon at the instant when



Flash Spectrum—taken with Slit Spectroscope.

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the spectrum is photographed. Then, if the atoms producing a certain line are distributed throughout the whole atmosphere, that line will appear as an image of the whole region *ABCD*. But if there is a line which arises only from the region below the dotted arc *EFG*, then that line will appear as an image of the smaller region *EFGD*, and so will have a shorter length in the spectrum. Obviously, the ends of each line will represent the highest points in the atmosphere at which the line is produced, and the longer the line, the greater will be the region occupied by the atoms giving rise to it.

Plate XXIII. shows the spectrum taken under these conditions by Fowler at the eclipse of 22nd January, 1898—the same eclipse as that which yielded Plate XXII., for which a slit spectroscope was used. The gradual disappearance of the continuous photospheric light and the simplification of the line spectrum as the Moon passed across the solar atmosphere will be observed. It is obvious that we have here a means of determining the chemical composition and physical condition of the various levels in the Sun's atmosphere. The evidence of the earlier method is confirmed, that hydrogen and helium (represented by the line, $\lambda 4471$) ascend to great heights, while the *H* and K lines of ionised calcium show the greatest range of all. Exact measurements show that the red hydrogen line ($H\alpha$) (outside the region of the spectrum shown in Plates XXII. and XXIII.) is produced at a higher average level than the other hydrogen lines. This is not surprising, because of the lines of hydrogen that reach us from the Sun it is the most easily stimulated. It would therefore be produced in the higher levels—where the temperature is presumably at its lowest—more copiously than the other lines. Hydrogen is not alone in showing this phenomenon. The several lines of the metals also represent different average levels, and thereby tell us something of the varying conditions existing in different parts of the Sun's atmosphere.

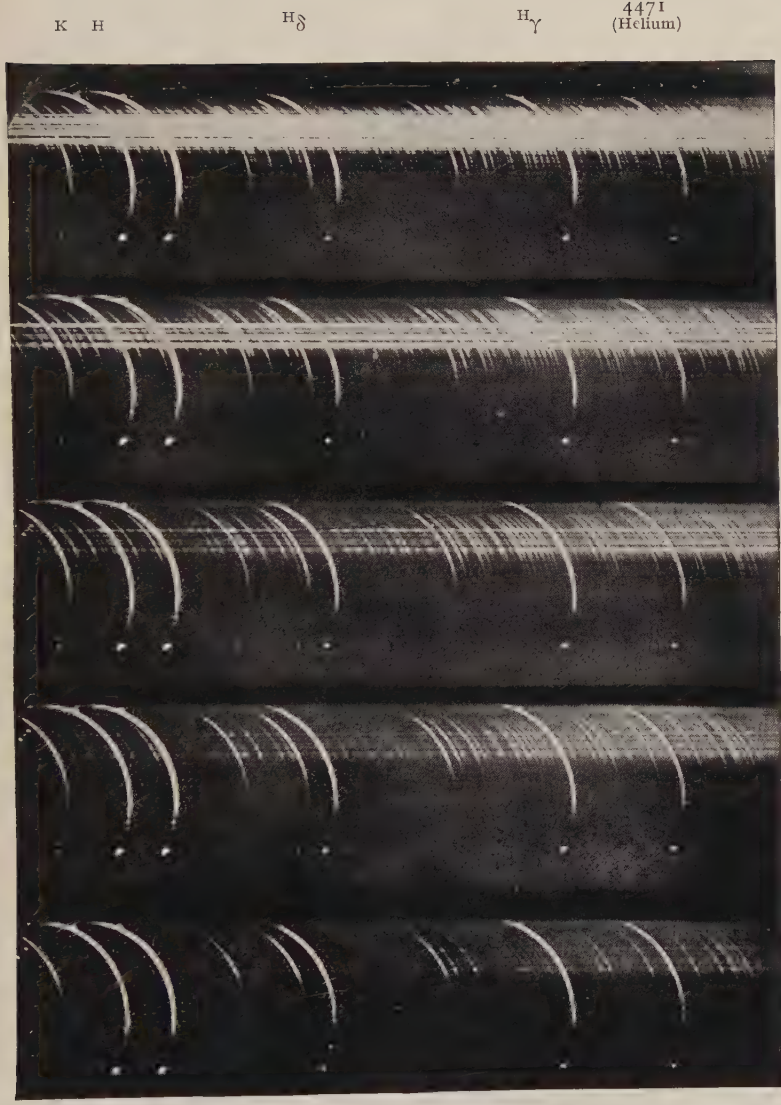
This method of obtaining the flash spectrum teaches us many things, but it labours under one great disadvantage: it allows, only with difficulty and with much liability to error, a measurement of the wave-lengths of the lines, which is important for the more subtle uses to which the spectrum is put. The lines are broad and uneven, and have unequal thicknesses: it is, moreover, impossible to obtain an adjacent comparison spectrum, since no slit is used. These disadvantages are serious, and make it necessary to supplement

the method by one which will permit of accurate measurements of wave-length. Such a method has been found in the use of a telescope giving a very large image of the Sun. When the image is large enough, it is possible to place the slit tangentially in such a way that it lies along the image of the atmosphere, without receiving any light from the brighter interior. In this way the chromospheric spectrum can be obtained at any time when the Sun can be seen. It is not necessary to wait for an eclipse—indeed, it is necessary that there shall be no eclipse, either by clouds or by the Moon. The method is not a substitute for the previous one, for it tells us nothing about the levels of production of the various lines. It is essentially a supplementary method, with the great merit that it affords opportunities for exact measurement.

One result of the measurement is the confirmation of the presumption that, generally speaking, the chromospheric spectrum is identical with the Fraunhofer spectrum, with the lines brighter, instead of darker, than the background. There are some differences, however—notably the appearance of bright lines of helium in the chromospheric spectrum which, as a rule, have no counterparts among the Fraunhofer lines. For some reason, helium does not visibly absorb the light of the Sun's interior (except on rare occasions, in the neighbourhood of sunspots, where it is probably heaped up), though it exists in the atmosphere and emits its own light strongly. Again, there are some differences of relative intensity between corresponding bright and dark lines, tending generally to the strengthening of enhanced lines in the chromospheric spectrum. These anomalies might perhaps find a partial explanation in the distribution of the atoms in the Sun's atmosphere. If a certain type of atom were distributed in a thin layer in the atmosphere, it would probably give a weak Fraunhofer line and a strong flash line. This distribution, moreover, is one which is quite likely to occur with some types of atom, which might be able to exist only within a narrow range of physical conditions.

The question of distribution brings us to another important point. We have so far considered the different levels of the Sun's atmosphere as if they were uniform throughout. It may be, however, that the conditions vary considerably at equal levels in different parts of the disc. The methods we have employed confine us necessarily to the parts of the

PLATE XXIII.



Flash Spectra at successive instants—
Taken with Objective Prism.

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atmosphere which happen to be at the boundary of the disc at the time we make our experiments. We must now see if there is any means of examining the atmosphere over the whole face of the Sun, and so of obtaining some idea of the distribution of the different kinds of atom horizontally as well as vertically.

A means of doing this occurred simultaneously to Hale and Deslandres, and was given practical effect by Hale in an instrument known as the *spectroheliograph*. The principle

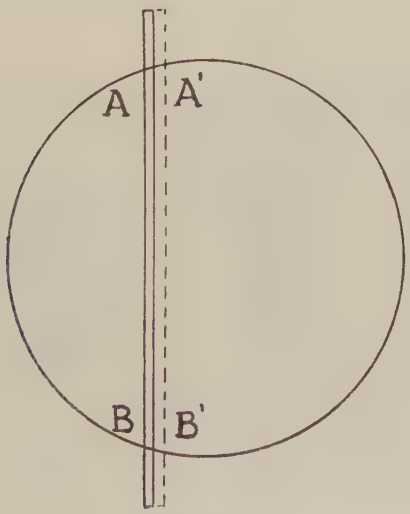


Fig. 15.

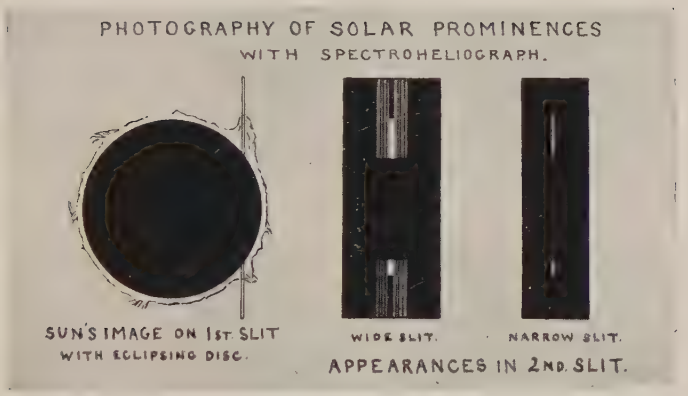
of the instrument is this. When the slit of a spectroscope is placed across an image of the Sun, the spectrum lines produced contain light only from that part of the Sun whose image is actually over the slit. Thus, when the slit is in the position *AB* (Fig. 15), the light in a given Fraunhofer line—say, the red hydrogen line, H_α—comes from the comparatively narrow strip, *AB*, of the Sun's atmosphere, and if it should happen that this particular strip contained no hydrogen, the line, H_α, would be missing from the spectrum, although hydrogen might abound in other parts of the Sun. Suppose, then, that we isolate the H_α line from the rest of the spectrum. It will then give us an actual picture, in red, of the masses of

hydrogen atoms in the strip AB which are in the act of emitting H_α. Now imagine the slit to be moved to the adjacent dotted position, and let the new H_α line, isolated from the rest of the new spectrum, be placed by the side of the old one. The new line will, similarly, be a picture of the same kind of hydrogen atoms in the region $A'B'$ of the Sun's atmosphere. It is easy to see that, by building up the whole image of the Sun in this way, strip by strip, in the light of a single line, we can get a representation of the distribution of the atoms emitting that line over the whole face of the Sun. Where such atoms exist, we shall have light: where they do not exist, there will be darkness.

This principle is put into practice in the following way. The image of the Sun is made to move steadily across a slit, so that the spectrum lines produced correspond to continually changing regions of the atmosphere. The Fraunhofer lines will, of course, remain stationary in the spectrum, but the intensity of the light in them, and its variations along the length of the lines, will change continuously, according to the distribution, in the Sun's atmosphere, of the atoms responsible for them. A second slit is placed across the line whose light is to be isolated, and the light passing through it is received on a photographic plate which moves in a direction perpendicular to the length of the line at exactly the same rate as the Sun's image passes across the first slit.[1] In this way, an image of the Sun is built up which shows the distribution of a particular type of atom in the atmosphere, and, by repeating the process with other lines, we can obtain a number of photographs showing the same thing for different kinds of atom. Each photograph represents what we should see if we could look at the Sun through a glass capable of transmitting light of the appropriate wave-length only.

Fig. 16, which is due to Fowler, shows clearly what is happening in the spectroheliograph in a particular case. (*a*) represents the image of the sun on the first slit at some definite instant. In the diagram, the central portion of the image is supposed to be covered by an opaque disc, so that light comes through the ends of the slit only, and corresponds to the bright radiations of the solar atmosphere. In the second slit, therefore, the spectrum line observed—which is

[1] In some forms of the instrument the two slits are made to move at equal rates, and the solar image and photographic plate are stationary.



(a) (b) (c)

Fig. 16.

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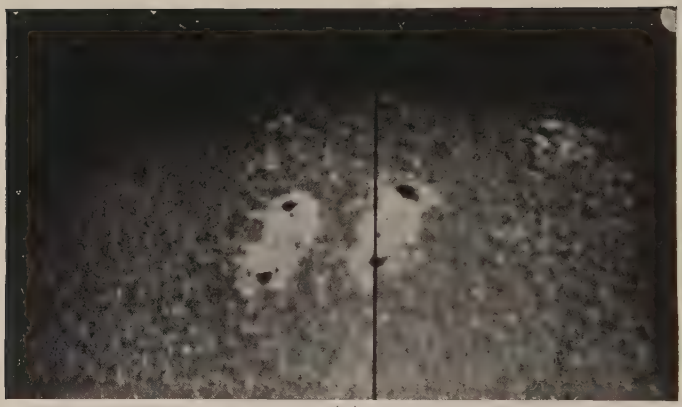
PLATE XXIV.



(*a*)

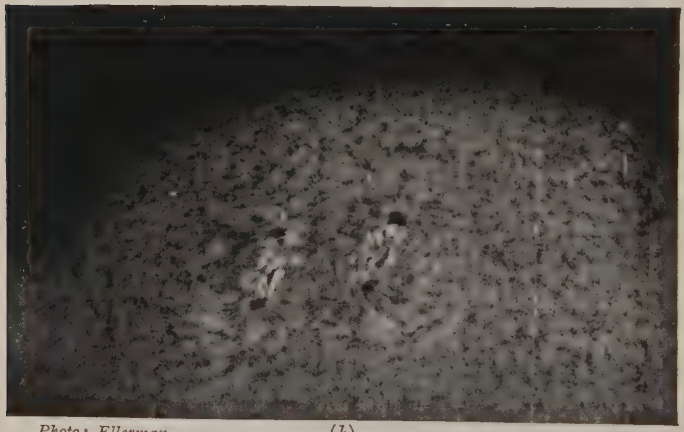


Photo: Ellerman. (*b*)

Spectroheliograms taken on 10th September, 1909.

(*a*) Calcium (H_2).
(*b*) Hydrogen (H_α).

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just an image of the portion of the Sun focused on the first slit—consists only of two short lengths separated by a dark space. This is shown in (*b*) and (*c*) ; (*c*) represents the line as it is ordinarily observed and photographed, and (*b*) shows the finite portion of the Sun which would be seen in light of a single colour if the two slits were widened, and which is actually photographed by moving the original solar image and the photographic plate relatively to the two narrow slits.

It is possible at present to use only the broadest lines in the spectrum. The fine ones are not able to cover the second slit without encroachment of the light from the photosphere. Plate XXIV shows spectroheliograms of the Sun taken, at the same time, in the light of the H_2 line of ionised calcium and the H_α line of hydrogen respectively. In each picture, the irregular structure of the disc, and the existence of nebulous masses known as *flocculi*, will at once be apparent. Equally striking are the differences between the two photographs. Not only is the distribution of the atoms completely different, but in the hydrogen photograph there is evidence of a circulation of material round the dark spots (sunspots), which is not to be seen in the ionised calcium clouds. We know from the investigations previously described that H_α is emitted from higher levels than H_2 (the highest atoms of ionised calcium are responsible for the fine dark line, H_3), so that apparently there is something in the Sun's atmosphere near a spot, causing a vortex motion, which operates only at very high levels. We shall return to this point—which is of very great importance—when we come to consider the nature of sunspots.

Having seen how the constitution of the Sun's atmosphere is investigated, we are now in a position to consider some of the problems which the results present. Apparently the most baffling, and therefore the most interesting, of these is the reason for the very curious manner in which the different elements are distributed with respect to level. In some measure allied to this is the question of why a number of terrestrial elements are not revealed at all in the solar spectrum. Let us look at some of the facts, in order more clearly to appreciate the nature of the problems. The following list shows the heights reached by a few of the metals in the Sun's atmosphere, as indicated by particular lines in their spectra :—

TABLE X

Element.	Atomic Weight.	Indicative Line.	Height reached.
Ionised Calcium	40	3968 – *H* 3933 – *K*	8750 mls.
,, Strontium	88	4216 4078	3750 ,,
,, Barium	137	4934	470 ,,
		4554	750 ,,
Neutral Calcium	40	4227	3125 ,,
,, Strontium	88	4607	220 ,,
,, Barium	137	5536	250 ,,

Hydrogen and helium are found at greater heights than any other substance except ionised calcium, which seems to reach to the very top of the atmosphere.

Two things are at once clear. If we consider ionised atoms quite apart from neutral ones, then the heights reached by the various substances are approximately in the order of the atomic weights; and, further, the ionised atom of an element appears to ascend much higher than the neutral atom. The first result is not surprising: we should expect the heaviest substances to be found at the greatest depths. But the second result seems to be inexplicable. Ionisation makes practically no difference to the weight of the atom, and yet we find, as a supreme example, ionised calcium, with an atomic weight of 40, ascending higher than hydrogen and helium, with atomic weights of 1 and 4 respectively. This is the first difficulty that calls for investigation.

The limitation apparently imposed on the number of elements represented in the spectrum offers a no less serious problem. Out of more than 90 elements in the chemist's table, only about 40 are known to show lines in the solar spectrum. It is remarkable that the average atomic weight of the elements giving lines is much lower than that of the missing ones—as if the Sun's atmosphere preferred light elements to heavy ones. Moreover, broadly speaking, the

lines of light elements are more intense than those of the heavy ones which do reveal themselves.

The explanation of these facts has long been elusive. Of course, we can, if we like, fall back on the contention that elements which give no Fraunhofer lines do not exist in the solar atmosphere, and that those whose lines are feeble are present only in small quantity. That is the simplest possible explanation, but astrophysicists have for long looked askance at it. It is generally believed that the Earth and the Sun once belonged to a single body, and it is hard to imagine how the Earth or the other planets could have captured the heavier elements so completely. Furthermore, some of the lightest elements show no Fraunhofer lines, and some—for example, nitrogen—are known to exist in the Sun's atmosphere in combination, and yet do not appear as elements in the spectrum. Again, if any one is bold enough to deny that an element can inhabit the chromosphere without giving spectroscopic evidence of its presence, he must face the chastening thought that helium, so prominent in the flash spectrum, shows no dark lines at all in the ordinary Fraunhofer spectrum. We must recognise, then, that the solution of our problem must be sought in a study of the conditions governing the distribution of the solar materials and the absorption and emission of particular spectrum lines. When this is realised, it becomes clear that the problem is ultimately the same as that of the levels occupied by the different elements in the Sun. The question of material distribution, and the power to emit or absorb, underlies them both.

A few years ago, Fowler put forward an ingenious suggestion which undoubtedly has some bearing on the matter. He pointed out, first of all, that the Fraunhofer spectrum is essentially an arc spectrum : that is to say, if we wish to repeat it in the laboratory, we must produce the conditions existing in the electric arc. It is true that it contains enhanced lines, which are characteristic, as a rule, of the spark, but the enhanced lines in the solar spectrum are those which can be produced in an ordinary electric arc or its equivalent, and those which cannot be so produced (for example, the line $\lambda 4481$ of ionised magnesium) are not to be found in the solar spectrum, although the arc lines of the corresponding elements may be represented there very strongly. It follows, then, that it is probably useless to search the solar spectrum for lines which cannot be produced under arc conditions. In particular, the

nitrogen arc spectrum contains no lines, but only bands, so that the absence of nitrogen lines from the solar spectrum is no evidence at all that nitrogen is not present in the Sun. It might mean that, if it is there, it is in the form of molecules, and not of atoms. Fowler draws attention also to the lower levels selected by the heavier elements, and suggests that the heaviest ones are probably too deep to give absorption lines at all—actually in the interior, where they contribute to the continuous background of the spectrum.

With regard to the relative heights attained by ionised and neutral atoms, he points out that we must seek for some agency which tends to repel an ionised atom from the Sun's centre, but which has no action, or a smaller one, on the neutral atom. Such an agency he finds in electrical repulsion. It is well known that a hot body discharges electrons, leaving itself with a positive electric charge. The interior of the Sun we know is at a high temperature, so that we may conceive of a positively charged layer at the boundary between the Sun's atmosphere and interior. An ionised atom also has a resultant positive charge, because it has lost an electron. Consequently, an ionised atom will be urged upwards, owing to the mutual repulsion of like charges, and will reach heights unattainable by a neutral atom of the same element. This electrical repulsion will act with the same strength on all singly ionised atoms, whatever may be their weights, but it will be able to push the lighter ones higher than the heavier ones, because the resistance of gravity will be less. Consequently, we have an explanation of the double fact that the heights reached by both ionised and neutral atoms are in the order of the atomic weights, and that ionised atoms ascend higher than neutral atoms of the same element. If it be asked why ionised atoms lighter than calcium—*e.g.*, magnesium—do not reveal themselves at still greater heights than ionised calcium, the answer is that, under arc conditions, ionised magnesium atoms do not give lines within the region of the spectrum we can examine. This is completely in accordance with experimental evidence.

It is very probable that the influences imagined by Fowler actually do exist and produce the effects he postulates. At the same time, his explanation is incomplete, inasmuch as it takes no account of factors which have since been shown to play a large, if not a leading, part in the matter. The chief of these depends upon the fact that various amounts of energy

are required to change atoms of different substances from one state to another (see p. 19)—in particular, from the neutral to the ionised state. This fact, indeed, dwells implicitly in Fowler's explanation, for it is partly responsible for the non-appearance of enhanced lines of magnesium under conditions which readily produce those of calcium. But it is only quite recently that the energy required to produce ionisation has actually been measured and applied to the problem we are considering ; and not only the energy needed for ionisation, but also the energy needed to change an atom from a state in which it can give one line to a state in which it can give another, in either a neutral or an ionised atom. From Fowler's point of view, an ionised atom was merely an ionised atom—a unit giving rise to any of the lines attributed to it. His analysis went but one stage farther than the chemist's ; he took account of ionisation, but of no other modification of the normal atom. The inadequacy of this outlook can be seen at once from Table X, in which it appears that the two lines representing ionised barium are produced in regions of greatly differing heights. Evidently, a complete explanation of the phenomena in question must reveal the cause of this, which is inexplicable so long as the different possible states of an ionised atom and the differences between their most suitable environments are ignored. The recognition of these differences is the characteristic feature of the modern outlook.

We owe to Dr. Megh Nad Saha the first clear expression of the new attitude to these problems. In brief, Dr. Saha's theory is this. The solar atmosphere consists of large numbers of atoms moving with high velocities and undergoing frequent collisions. As a result of some of these collisions, electrons may be detached from atoms, leaving the atoms ionised : as a result of further collisions between electrons and ionised atoms, the electrons may be caught again, and the atoms would then resume their neutral state. In the general case, any particular region of the atmosphere will contain neutral atoms, ionised atoms, and electrons. There will be frequent dissociation and recombination between electrons and atoms, but, if the physical conditions remain constant, a constant proportion of neutral and ionised atoms will, on the whole, be maintained. What this proportion will be will depend on the physical conditions and the energy required to ionise the atoms present. By mathematical calculation, Saha has shown that the conditions favouring ionisation are

high temperature and low pressure, while against the influence of these conditions we have the tenacity with which the atom of an element holds its electron. The last-named quantity is measured technically by what is known as the "ionisation potential" of the element. We cannot here explain exactly what this means, but it will suffice for our purpose to know that the higher the ionisation potential, the greater is the energy necessary to ionise an atom. Each element has its own ionisation potential.

It will be clear now why, in certain regions of the atmosphere, some elements show enhanced lines while others do not. In any given region, the physical conditions are the same for all the elements present, but the elements are ionised in differing degrees because of their differing ionisation potentials. On the other hand, a single element will be ionised in differing proportions at differing levels, because the physical conditions vary from level to level: the higher we ascend, the lower becomes the temperature and the lower the pressure. It is a matter of mathematical calculation to determine the percentage amount of ionisation experienced by any element at any particular level—a calculation made possible by our more or less accurate knowledge of the variation of temperature and pressure throughout the Sun's atmosphere, and laboratory researches into the ionisation potentials of the elements.[1] Calculations made on these lines actually show that, in the main, the occurrence of the spectrum lines is in accordance with the theory—at least, so far as the relations between ionised and neutral atoms are concerned. Above a certain level, ionisation of a particular element may be complete. No lines of the neutral atom of this element can therefore appear there. The calculated positions of the levels for various elements are, in general, in agreement with observation.

[1] Saha gives the following formula for calculating the fraction (x) of the atoms of an element which, in the absence of other elements, are ionised at a pressure, P atmospheres, and an absolute temperature, $T°$:—$\log\left[\frac{x^2}{1-x^2}.P\right] = -\frac{5036I}{T} + \frac{5}{2}\log T - 6.5$; where I = ionisation potential of the element. Russell has generalised the equation to meet the case of a mixture of elements. If $\bar{x}$ be the fraction of all the atoms present which are ionised, irrespective of their chemical nature, and x_1 be the fraction for any one of the elements whose ionisation potential is I, then the left-hand side of the equation becomes $\log\left[\frac{x_1}{1-x_1}.\frac{\bar{x}}{1+\bar{x}}.P\right]$.

But perhaps the outstanding merit of the theory is that it shows us how to attack the subtler problem of why the different lines produced by the same type of atom reach different heights in the chromosphere. We saw in the first section that, in order to radiate a given line, the outermost electron of an atom must fall from a certain specified orbit, *a*, to an inner orbit, *b*. Since, in the normal state of the atom, the electron is in the innermost orbit of all, it must be transferred to the orbit, *a*, before the line in question can possibly be emitted. Such a transference is a step towards ionisation, and will be brought about by the same physical conditions, acting in different degrees. Consequently, we can apply the above methods to calculate the percentage number of atoms of an element which are in a condition to produce any particular line, provided we know the potential corresponding to the transference in question instead of to ionisation. This knowledge involves a considerable amount of laboratory research which, for some elements, is simplified by certain spectroscopic data.

There is an important omission from Saha's theory, which prevents exact agreement between the predictions and the facts. The change of state of an atom is brought about, not only by the physical conditions in the atmosphere, but also by the absorption of light from the interior. We have seen (p. 23) that absorption must involve the removal of an electron to an outer orbit. This has not been taken into account in the theory. Moreover, the possible influence of one element on another in a mixture of substances has been ignored. Russell has considered this matter, however, and has carried the theory a step further towards completion. While, in the main, the theory agrees well with observation, it is to be regarded at present as an approximation to the truth, and much work will have to be done before it reaches its final state. But meanwhile, it points the way to further researches, and there can be no doubt that it is fundamentally on the right lines.

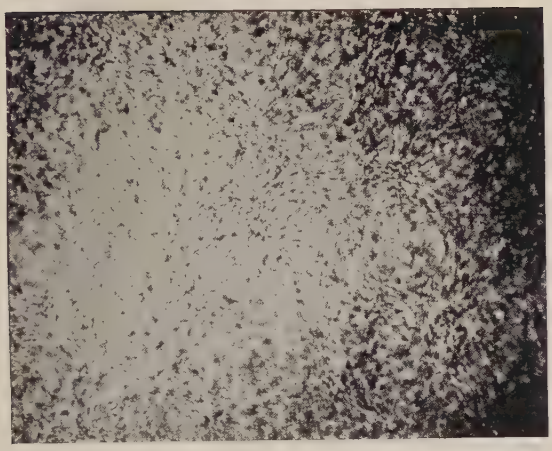
In the Sun's atmosphere, then, we are able to trace the operation of certain physical conditions, the detection of which will help to enlarge our knowledge of the stars in general. We must now turn to the interior of the Sun, and see what we can learn there. And here we come at once to a problem about which there is still considerable difference of opinion.

What is the nature of the discontinuity between the atmosphere and the interior of the Sun? In other words, what is the photosphere? If we look at the Sun with high magnifying power we see what is apparently a mottled surface (see Plate XXV). At times it is infested with spots, and when the spots are near the limb of the Sun, they are usually attended by exceptionally bright patches, known as *faculæ*. But the greater part of the disc presents this mottled appearance, which is in a continual state of change. No two photographs, however rapidly one succeeds the other, show the same mottlings. It is the nature of this ever-changing phenomenon, which we call the photosphere, that presents so difficult a problem.

The various ideas on the subject can be classified into two distinct types. According to one view, the photosphere is an actual surface—probably liquid or cloudlike, but perhaps in some physical state that we cannot match under terrestrial conditions—at any rate, an opaque layer, definitely located at the base of the Sun's atmosphere. The other idea regards the Sun's interior as gaseous throughout. It is, in that case, impossible to assign a definite position to the photosphere: what we see is the light that finally emerges from the whole of the gaseous globe. That there is a clear issue between the two views may be seen at once by considering the effective temperature of the Sun. According to the first view, the effective temperature is definitely the temperature of the photospheric layer: according to the second, it is indefinite; we cannot assign to any *particular* region of the Sun a temperature equal to the effective temperature. It should perhaps be mentioned that there is a third suggestion, that the appearance of the photosphere is really a will-o'-the-wisp—an optical illusion arising from the manner in which the Sun's light is refracted through the various solar layers it encounters as it travels outwards. This view has never found much favour with actual observers of the Sun, and, in the limited space at our disposal, we shall take no further account of it.

One thing is certain, namely, that there is a sharp discontinuity at the base of the Sun's atmosphere. The sudden change from a bright continuous spectrum to a much fainter line spectrum is conclusive evidence of that. Moreover, we know from Kirchhoff's fundamental principle (p. 15), and also from actual measurement, that the photosphere, whatever it be, is hotter than the atmosphere. These are

PLATE XXV.



(*a*)

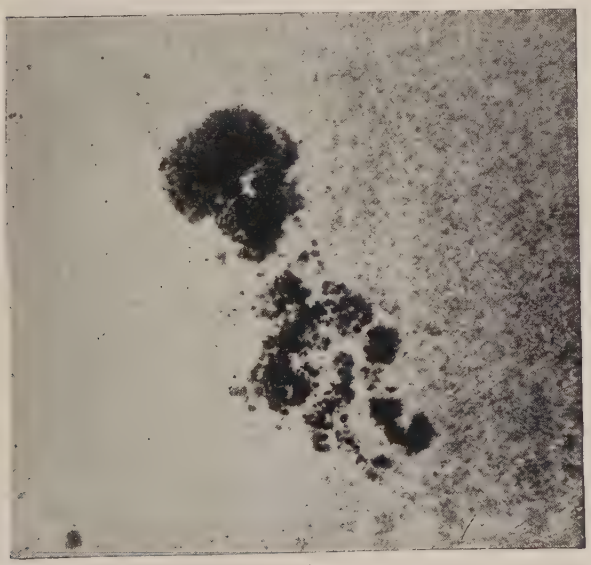


Photo: Janssen. (*b*)

Photographs of the Sun's Surface.

(*a*) Photosphere.
(*b*) Group of Sunspots.

To face page 200.

ascertained facts. Beyond them, all is at present conjecture, with a greater or less amount of probability. Let us review the chief arguments in favour of the two ideas.

First of all, the layer theory has appearances in its favour. The photosphere looks for all the world like a boiling liquid surface. If it were so, we should expect the irregularities of level to show themselves as the continual flux of mottlings that we see. Moreover, the image of the Sun shows a very sharp boundary—a natural consequence of a photospheric layer, but a surprising thing if the Sun's interior is gaseous throughout. Again, the spectrum of the photosphere—a continuous one—is the spectrum invariably given by a glowing solid or liquid, whereas an incandescent gas usually gives a line spectrum.

These are strong points, but the gaseous theory can advance arguments which are equally strong. Against the liquid or cloud hypothesis there is the damning fact that all known elements boil, and therefore evaporate, at a temperature not much above 3000° C., whereas different methods of investigation agree in placing the effective temperature of the photosphere in the neighbourhood of 6000° C. It has been urged, in explanation of this, that the pressure in the photosphere is much greater than the pressure in the Earth's atmosphere, and it is well known that increase of pressure raises the boiling point of a substance. A very large increase of pressure, however, would be necessary to account for the difference between 3000° and 6000°—an increase which does not seem probable from the evidence available. It is now generally admitted that the pressure in the Sun's atmosphere is very low—approaching what we call " vacuum " conditions on the Earth. As a single example, certain magnesium lines (*e.g.*, λ4167, λ4058), which are quite sharp in the solar spectrum, are very broad and diffuse in the electric arc in air. It is not until the pressure in the arc is lowered almost to nothing that these lines can be produced in the laboratory with a sharpness comparable with that which they show in the Sun. And these are lines which occur at fairly low levels in the Sun's atmosphere. It is not easy to believe that the pressure can change between their place of origin and the base of the atmosphere, sufficiently to cause condensation at the high temperature which the photosphere is found to possess.

This is a strong argument in support of the gaseous theory —almost it seems, a conclusive one. There are, however,

difficulties to be faced. Why, if the Sun's interior is totally gaseous, does it show a continuous spectrum and a sharp boundary? The continuous spectrum is accounted for by the fact that the interior of the Sun is under great pressure, and at a high temperature. Under these conditions—and, indeed, at low pressures and temperatures, in certain special circumstances which are not yet fully understood—gases are known to give a continuous spectrum. Obviously, then, if the interior of the Sun is wholly gaseous, it can, under the conditions which obtain in it, give no other than a continuous spectrum.

The sharp boundary of the Sun offers a more serious problem. It must be understood, however, that the sharpness may be only apparent. It is not at all necessary to suppose that there is anything like so sharp a transition from atmosphere to interior as the existence of condensation would imply. If the brightness of the interior needed a layer 500 miles thick, in which to fade out to invisibility, this layer would subtend an angle of only one second of arc at the distance of the Earth, and the boundary of the Sun would appear perfectly sharp. An explanation of this apparent sharpness has been put forward by Abbot, the champion of the gaseous theory, which depends upon the scattering of light by small particles (see p. 175). He points out that, in looking at the limb of the Sun, we are looking through the greatest depth of atmosphere, and as we move our eyes from the centre towards the limb, the *rate of increase* of thickness of the effective atmospheric layer that we look through grows greater and greater. When we are actually looking at the limb, the tiniest movement of our line of sight from side to side corresponds to a very great change in the effective thickness of the intervening atmosphere. If, now, the Sun's atmosphere scatters the light of the interior—as it might very well do—the amount of scattering will increase very quickly towards the limb, so that the apparent brightness of the photosphere will there diminish very rapidly with increasing radius of the disc. As the result of calculations based on certain reasonable assumptions, Abbot shows that this effect is sufficient to account for the sharpness of the Sun's apparent boundary.

There remains yet the curious mottled appearance of the photosphere; how is that accounted for on the gaseous theory? A detailed explanation is not forthcoming, but

we know from independent evidence—particularly the phenomena of sunspots—that there are considerable irregularities of temperature in the photosphere. We should expect, then, that there would be irregularities of brightness, and though we cannot predict what types of irregularity should present themselves, the observed mottling would be a very probable consequence. The gaseous theory inevitably suffers from the disadvantage that the conditions which it postulates cannot be reproduced in the laboratory. Its phenomena arise largely from the very vastness of the scale on which the processes take place; and whereas a large glowing liquid would be very much the same in appearance as a small one, the appearance of a globe of gas so large and hot as the Sun cannot possibly be imagined clearly by beings whose experience is so limited as ours. This has to be borne in mind in considering the relative merits of the theories.

But perhaps the most difficult problem which the gaseous theory encounters is the explanation of the fact that there is any discontinuity at all between the interior and the atmosphere of the Sun. In a wholly gaseous mass, one would expect the conditions to change, indeed, from the centre outwards, but to change continuously, however quickly or slowly they might do so. The Wolf-Rayet stars seem to be the only ones that behave as a wholly gaseous mass should. Their spectra are intelligible—a faint continuous spectrum due to the light from the high-pressure interior which has managed to escape, with a bright line spectrum from the surface layers impressed on it. But these stars form an extremely small proportion of the whole number. The overwhelming majority, like the Sun, show absorption lines, which must indicate an abrupt change of condition at a certain level. Why there should be this change in nearly all the stars is a serious question for the gaseous theory.

It is a question, further, which has received no final answer. Supposing the Sun to be gaseous throughout, we do not know why, at a certain level, it changes its properties so suddenly. But, at the same time, this is not to be used as an argument against the theory. We do not know why the change occurs, but we do know that something similar can occur, for we see it in the electric arc. Every one who is familiar with the light of the iron arc, for example, knows that it consists of a central core of blue light, surrounded by a pale reddish flame which adjoins the core discontinuously. Moreover, the outer

flame produces absorption lines—*reversed* lines (see p. 16)—exactly like the Fraunhofer lines in the solar spectrum. With regard to some of the spectroscopic phenomena which it shows, the electric arc is a model of the Sun, but no one suggests that the arc contains a "photospheric layer." There is actually an abrupt change from a hot gas to a much cooler one, and though we do not know the cause of it, we are bound to admit its existence.

On the whole, it appears that the gaseous theory, at present, has the last word. As compared with the layer theory, it is not so much that the points in its favour are stronger as that those against it are weaker. The layer theory has yet to explain satisfactorily how condensation of any kind can take place at the measured temperature of the photosphere. Until it can do that, it seems destined to remain under grave suspicion. The gaseous theory, on the other hand, may not be able to boast its virtues, but it can at least face the world with a clear conscience: there is no unpardonable offence laid to its account.[1]

The question of the Sun's effective temperature assumes a special interest in view of the mystery surrounding the nature of the photosphere; for there is a *prima facie* possibility that, if different methods of determination should give different effective temperatures, the discrepancies might be traced to differences in the solar regions to which the several results refer, and so demonstrate the truth of the gaseous theory. The effective temperature of the Sun can be calculated by the methods previously mentioned (Chap. IX.), which depend on the wave-length of maximum energy in the spectrum or the distribution of energy throughout the whole spectrum. It can be calculated also from the amount of heat which would be received from the Sun at the Earth's surface per unit area per unit time, if the Earth's atmosphere were removed. This quantity, which is known as the *solar constant*, can be measured by certain special means, and it is a simple matter to calculate from it what the total radiation of heat from the Sun must be. Now, if the Sun is a perfect radiator, its rate of radiation is proportional to the fourth power of its absolute temperature, which therefore becomes calculable. We have here, then, a quite distinct method of

[1] An interesting discussion bearing on this question will be found in the lecture by Milne referred to on p. 116.

measuring the Sun's temperature, which has, in common with the previous methods, only the assumption that the Sun is a perfect radiator.

The results given by the different methods are not the same. The effective temperature determined from the spectrum is in the neighbourhood of 6500° absolute: the solar constant method gives 5860° absolute. If we were sure that the Sun were a perfect radiator, we should have to conclude from this that the heat and the light radiation take place effectively from different regions, and the conclusion would give some support to the gaseous theory. But we are not sure of this: the evidence, in fact, is in favour of the view that the Sun is not a perfect radiator, and we know that any departure from perfect radiation would result in the temperature calculated from the solar constant being too low. The only definite conclusion we can reach, then, is that the effective temperature of the Sun is probably greater than 6000° absolute, but what part of the Sun actually has this temperature is an open question.

It might be well to observe that, whatever the photosphere might be, the result must certainly be influenced to some extent by the unknown amount of scattering of radiation in the Sun's atmosphere. This would be greatest near the limb of the Sun, and, as a matter of fact, the energy distribution in the light from the limb gives a lower temperature than that in the central light. Further, photographs of the Sun show that the brightness also falls off at the limb. According to the gaseous theory, this might occur, apart from atmospheric scattering altogether, on account of the fact that, at the limb, we look through a smaller depth of the Sun's interior. Exactly how much effect this would have on the amount of the radiation, depends on the character of emission of light from a globe like the Sun. The radiation would probably occur somewhat in the following manner. From the centre—the hottest point—there would be radiation in all directions. The innermost layers would receive this radiation, absorb—or perhaps scatter—some of it, and transmit the remainder, with their own radiation added, to the next layers. The same process would be repeated from layer to layer, until, at last, the light which would finally emerge in any direction would not be traceable to any particular layer, but would be made up of that which had found its way to the surface from various depths, plus a quantity of scattered light originally propagated in quite different directions. How, in the

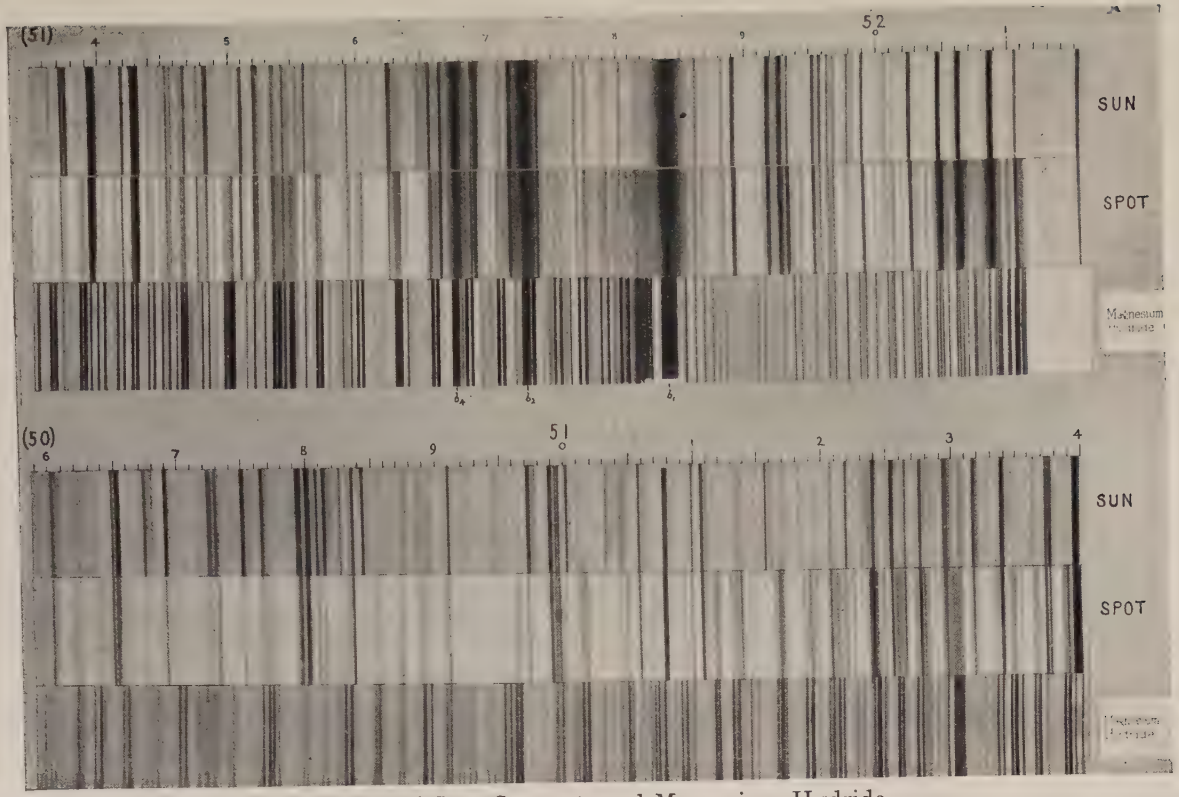
spectrum of such a complexity of radiation, the energy distribution would be related to the huge range of temperature from the Sun's centre to its surface is indeed a difficult problem. It may be doubted whether the relation would have much analogy with that observed when an isothermal surface is the radiator.

It seems clear, then, that no visible light can at present be thrown on the problem of the photosphere by investigations of the Sun's effective temperature. We turn now to the phenomena which, rightly or wrongly, have for long been held by astronomers to contain the key to many of the solar mysteries—namely, the spots which come and go on the Sun's disc. A typical sunspot (see Plate XXV.) consists of a dark area (the *umbra*), surrounded by a region (the *penumbra*), which is intermediate with respect to brightness between the umbra and the general photosphere. Spots appear fairly suddenly, often in groups, and seem to move across the Sun's disc in such a manner as to show that the Sun, like the Earth, rotates on an axis. A particular spot remains in existence for a period which may be anything from a few days up to several months.

The spectrum of a sunspot resembles that of the general surface of the Sun in that it is of the absorption type, but there are marked differences.[1] This can be seen at once in Plate XXVI. Without entering into the details of the differences, we may say that they point consistently to the conclusion that the temperature of the vapours over a spot is lower than that of the rest of the Sun's atmosphere. In the spot spectrum, high temperature lines are weakened, or disappear, while low temperature lines are strengthened. Moreover, the bands of compounds, such as magnesium and calcium hydrides and titanium oxide, are to be found there—compounds which, at the ordinary chromospheric temperature, could not exist. There is no doubt, therefore, that the region of a sunspot is a cooler region than the rest of the Sun. This is an important fact, to which we shall refer again.

Both before and since the interpretation of the solar spectrum by Kirchhoff and Bunsen, sunspots have been the centre round which most of the controversy respecting the

[1] Sunspots, like Fraunhofer lines, are only *relatively* dark: they are actually very bright, and yield a spectrum which can be observed without difficulty.



Spectra of Sun, Sunspot, and Magnesium Hydride.

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constitution of the Sun has been carried on. What is their nature? What level in the Sun do they occupy? How do they arise? These are the questions which every solar investigator has put to himself and attempted to answer. Looking back from our present position, we can see that, until fairly recently, all attempts to answer these questions were bound to fail, because knowledge which belongs to the very heart of the matter was completely hidden. It would be presumptuous, even now, to say that we know precisely what a sunspot is, but it is becoming more and more evident that we have discovered the real starting point of the investigation, and can advance, not indeed without caution, but with the conviction that we are going in the right direction.

To understand the present view of sunspots, we must return to Plate XXIV, in which we saw that the vapours at the highest levels surrounding a spot show a circulatory or vortex motion which is not to be seen in the lower vapours. This was first observed at Mount Wilson in 1908, and it at once suggested to Hale the idea that gave to Astrophysics a totally new outlook on the question of sunspots. We mentioned just now that a hot body discharges electrons from itself, so that, in the atmosphere of the Sun, there must be swarms of free electrons. If, now, these electrons take part in the vortex movement of the high level vapours round a spot—as they probably do—we must have what is virtually a circular electric current, for electrons in motion are equivalent to such a current. But a circular electric current is equivalent to a magnet in which the lines of force are perpendicular to the plane of the current. Consequently, there should be found in a sunspot a magnetic field, having its lines of force normal to the face of the Sun.

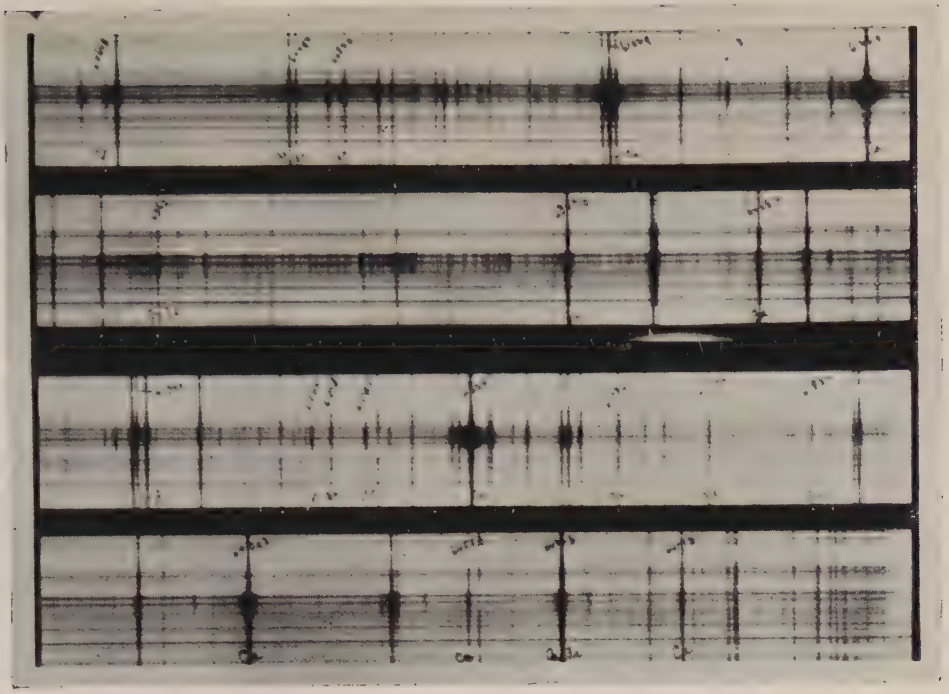
This was Hale's line of reasoning, and he at once set to work to look for a magnetic field. The ordinary laboratory methods of detection—depending on the force exerted by a magnet in the surrounding space—were not nearly delicate enough for his purpose, because of the enormous distance between the Sun and the Earth. But there was a spectroscopic method—the use of the Zeeman effect (see p. 39)—which was independent of the distance of the Sun, and which would answer his question immediately if the magnetic field were powerful enough to give an observable splitting of the spectrum lines. Accordingly, he made a careful examination of the spectrum lines in sunspots, both in the centre and at the

edge of the Sun's disc (the supposed magnetic field would be respectively parallel and perpendicular to the line of sight in the two observations), and showed, beyond doubt, that a magnetic field actually did exist in a sunspot, with an intensity sometimes of 4500 gausses—9000 times as powerful as the Earth's field. The effect was shown best by extinguishing the components of the dissected lines in turn by means of a polarising apparatus. The slit was divided into a number of parts along its length, and the components in the corresponding parts of each spectrum line were extinguished alternately, their actual existence being revealed by the jagged appearance of the line (Plate XXVII.). It appeared that, generally speaking, the strength of the magnetic field increased with the diameter of the spot, though there was a maximum strength corresponding to spots smaller than the largest observed. Hale found also that the *polarity* of the spots (*i.e.* the direction of the lines of force, whether towards or away from the centre of the Sun) varied in a manner which was later found to be perfectly regular.

Here, then, is a basis for a theory of the nature of sunspots. It is supposed that, for some reason which we do not yet understand, an internal eruption of the Sun produces a sudden uprush of gases. Coming quickly into the rarer atmosphere, these gases expand, and thus fall rapidly in temperature, causing an apparent darkening of the photosphere at the place of disturbance. Laboratory experiments show that free electrons tend to move from a hotter to a colder body, so that the electrons in the Sun's atmosphere would be drawn towards this cooler region. They are supposed there to be set in violent revolution, in just the same way as a vortex motion is produced in a waterspout on the Earth, which bears a considerable resemblance to a sunspot on this hypothesis.

The facts, so far as we have observed them, are consistent with this idea. Looking on a sunspot as a solar waterspout—in shape something like a huge mushroom—the penumbra corresponds with the projecting top, and the umbra with the stem. At the top, vapours should be sucked into this tremendous whirl, and masses of glowing hydrogen have actually been seen to move into it. Moreover, the researches of Evershed and St. John show that, at high levels, the motions of the vapours in a spot are inwards and downwards, whereas, at low levels, the motions are upwards and outwards—exactly as they should be according to the hypothesis. Finally,

PLATE XXVII.



Sunspot Spectrum.

Showing Existence of Magnetic Field in Sunspot.

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to establish the theory on still firmer ground, Hale has been able to detect "invisible" spots, by means of the Zeeman splitting of the spectrum lines, in places where there is no darkening of the photosphere. This, he says, "confirms the view that a spot represents a vortex which becomes visible only when the cooling due to expansion is sufficiently great to produce a perceptible decrease in the brightness of the photosphere."

The researches at Mount Wilson which have followed Hale's discovery are full of significance. It was noticed long ago that sunspots tend to occur in pairs, one following the other in their journey round the Sun's axis, and it appears now that this state of affairs is much more general than was at first suspected. Single spots are often accompanied by "invisible" companions, and it is believed that the double spot is the normal phenomenon. Accepting this, at least as a working hypothesis, the remarkable fact comes to light that the two members of a double spot invariably show opposite polarities; *i.e.* regarding the spots as magnetic poles, one member is a N-pole and the other a S-pole. Not only so, but—except at certain special periods to be mentioned later—all the pairs of spots in either hemisphere of the Sun behave alike with regard to polarity, and in the opposite manner to the spots in the other hemisphere: thus, if in the northern hemisphere the "preceding" spot of one pair is a N-pole, the preceding spots of all pairs in that hemisphere are N-poles, and the preceding spots of all pairs in the southern hemisphere are S-poles. The "following" spots have, of course, the opposite polarity to the preceding spots in all pairs. There are two possible explanations of difference of polarity in spots: the revolving electrified particles may have the same kind of electric charge and be revolving in opposite directions, or they may have opposite kinds of charge and be revolving in the same direction. There is no certain evidence as to which alternative gives the true explanation, but Hale inclines strongly to the former. If he is right we have further support for the vortex theory, for it is known that, on the Earth, cyclones and tornadoes have opposite directions in the two hemispheres.

With regard to the actual charge of the revolving particles and the direction of their revolution, we are still in the dark. It might appear that the direction of revolution (and hence, when the polarity of the spots is taken into account, the

nature of the predominating electric charge) is sufficiently revealed by photographs of hydrogen flocculi such as Plate XXIV. In all likelihood, however, these flocculi, although they led to the discovery of the magnetic fields, are not the vortices which give rise to the fields themselves. Hale thinks that the field-producing vortices may be within or beneath the photosphere and so out of the reach of direct observation. In that event, the high-level hydrogen vortices might be independent of them so far as direction of revolution is concerned, and so throw no light on the problem before us. It is curious that, through an excess of justification, so to speak, Hale's original line of argument from vortex motions in flocculi to magnetic fields has put itself out of court.

We have at last, then, a consistent and comprehensive theory of sunspots. We have some idea of what they are, and to what levels in the Sun they must be assigned. We do not, however, know how they arise. We have, at present, no glimpse of the causes which underlie the original vortices and uprush of hot gases into the cooler atmosphere. It might be thought that we could hardly expect to know this ; that in such a huge and exceedingly hot body as the Sun, eruptions on an enormous scale might take place at any time and at any place, and be as incalculable as the results of the throwing of dice. But, in point of fact, this is very far from the truth. There is a regularity about the appearance of sunspots, both in time and in place, which is as striking as, before its discovery, it was unexpected. To cut a long and interesting story short, we will merely state the bare facts. If the number of spots on the Sun—or, better still, the total of the areas covered by them—be observed for a number of years and plotted on a diagram such as that in Fig. 17, it will be noticed that there is a marked periodicity. The spots reach a maximum approximately every eleven years, and show minima at unsymmetrically intermediate times. Moreover, the regions in which the spots occur change with a similar regularity. Confined always (with very few exceptions) within two belts extending in solar latitude from 5° to 40° one on each side of the equator—the spots, a little before the time of their minimum frequency, begin to break out at the higher limit of latitude in each hemisphere. The region of prevalence gradually moves towards the equator, and continues to do so fairly regularly for about twelve or fourteen years. After about eleven years, however, a new cycle begins at the higher

latitudes, so that, about the time of minimum, there are four spotted regions, one at each limit of the infected belts. The cycle of changes is shown clearly by Fig. 17.

The study of the magnetic fields in sunspots has shown us a new aspect of these remarkable phenomena. At the sunspot minimum of 1912 it was noticed that the spots of the new cycle, which were just appearing in high latitudes, showed opposite polarity to those of the old cycle. Thus,

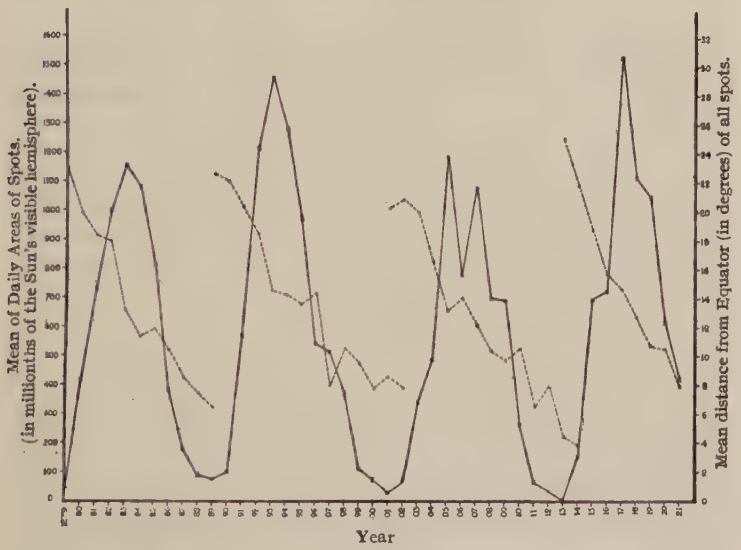

Fig. 17

Graphs showing variation of area (continuous graph—scale on left) and latitude (dotted graph—scale on right) of Sunspots.

while in the old cycle the " preceding " spots in the northern hemisphere were S-poles and those in the southern hemisphere were N-poles, the new cycle consisted of pairs whose preceding spots were N-poles in the northern hemisphere and S-poles in the southern hemisphere. The " following " spots continued to be opposite in polarity to the preceding ones in all pairs. This observation was totally unexpected. The cycle then beginning has maintained its new polarity to the end, and now in 1923, when another cycle is just beginning, we find it showing a return to the polarity which was

abandoned in 1912. It appears that we must reckon with a reversal of polarity at the beginning of every cycle, so that the true sunspot period—*i.e.* the interval of time occupied by a complete set of the periodically recurring phenomena—may be twenty-two instead of eleven years. In any event, we have now two criteria for distinguishing spots of one cycle from those of another during the times when both sets are present together; we have differences of latitude and of polarity, and there should therefore be no difficulty in assigning a spot to its true cycle.

The cause of these remarkable changes is totally unknown. Various attempts have been made to discover it, but without success. The eleven-year period is followed remarkably closely by a number of terrestrial phenomena, the consideration of which, however, hardly belongs to our subject. The Sun is found to give 3 or 4 per cent. more heat at the time of sunspot maximum than it does at the time of minimum. For this also no cause can be assigned, though it is evidently connected with the general cycle of solar activity of which sunspots appear to be only a symptom. We believe we have discovered what a sunspot is, but why it should come into being is a complete mystery.

It is to sunspots that we owe our first intimation that the Sun rotates on an axis. Groups of spots, preserving a nearly constant configuration among themselves, appear to move steadily across the Sun's disc from east to west. The most probable explanation of this is that the spots are fixed on the surface of the Sun and are being carried round by the Sun's rotation. This view is confirmed by the Doppler effects shown by the light from opposite limbs, which we have already noted in connection with the detection of telluric lines in the solar spectrum. The direction of the Sun's axis is very nearly normal to the plane of the Earth's orbit.

Obviously, it is possible to measure the rate of rotation, either by observing the period between two successive passages of the same spot across the centre of the Sun's disc (making allowance, of course, for the movement of the Earth in its orbit during the period), or by finding the velocity at a point on the limb from the Doppler effect, and dividing it into the circumference at the latitude of the point. The results given by the two methods agree very well, but they bring to light a very remarkable fact, namely, that the period of rotation of the Sun varies with latitude; in other words, the Sun does not

PLATE XXVIII.

Fig. 1. Mar. 4, 1910. −13 West.

Fig. 2. Mar. 17, 1910. −2 East.

Fig. 3. Nov. 19, 1908. −25 East

Fig. 4. Mar. 17, 1909. +14 East.

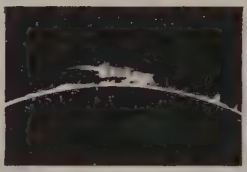

Fig. 5. Mar. 16, 1909. +11° East.

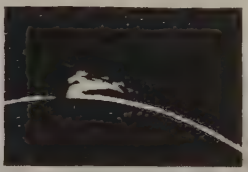

Fig. 6. Oct. 27, 1908. −13 East.

Fig. 7. Sep. 22, 1909 +10° to −10° West.

Fig. 8. Jan. 15, 1909. +33 West.

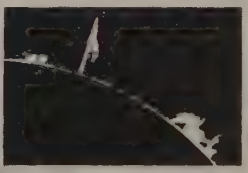

Fig. 9. April 21, 1909. +20° West.

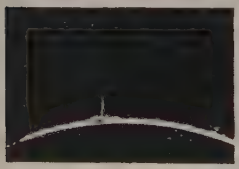

Fig. 10. Nov. 30, 1909. +11° East.

Fig. 11. Mar. 24, 1909. −25° West.

Fig. 12. May 2, 1907. −15° East.

PROMINENCES OF VARIOUS FORMS.

Figs. 1 and 2. A very stable prominence which was photographed alternately on limb and disc for three months. Fig. 3. An unusually bright prominence. Figs. 4, 5, 6. Broad massive forms, composed chiefly of horizontal filaments. Fig. 7. Prominence with sharply defined boundary on northern side. Figs. 8, 9. High narrow forms. Fig. 10. Eruptive prominence over a sunspot. Fig. 11. Broad prominence supported on slender columns. Fig. 12. Diffuse prominence of unusual form.

Photo. Engraved & printed at the Office of the Survey of India, Calcutta, 1916.

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rotate as a rigid body, but as a fluid acting under influences which we are not yet able to discover. The rotation period at the equator is about 24.5 days, but the period increases considerably in higher latitudes, both north and south. Carrington, from observations within the sunspot region, found a mean period of rotation of 25.38 days: it appears that the slowing down continues consistently up to the poles. The cause of this phenomenon is another of the mysteries which the Sun delights in presenting to us.

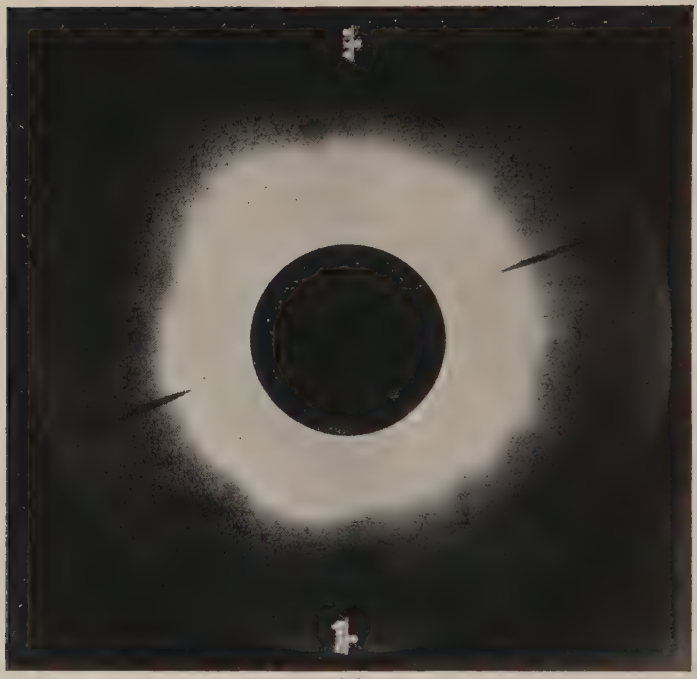
The discovery that the Sun rotates, however, has brought about the detection of another of its multitudinous activities. Problems arising from the Earth's magnetism have led physicists to ask themselves the question: "Is every large rotating body a magnet?" From this general question it is but a step to the particular one: "Is the Sun a magnet?" After his discovery of magnetic fields in sunspots, Hale set himself to attack this larger problem. It was obvious at once that there was no practicable test for solar magnetism but the Zeeman effect, and it was very doubtful whether even that was sufficiently delicate, seeing that, for decades, the solar spectrum had been examined closely by observers of the first rank, without a suspicion of the Zeeman splitting of the lines having arisen. Investigations conducted in the most careful manner, however, have convinced Hale and his assistants that the Sun actually is magnetised like the Earth, its magnetic axis being almost, if not quite, coincident with its axis of rotation. The effect on the spectrum lines is extremely small; even with the best of modern instruments, there is no actual separation of the components of the lines, but merely an overlapping, which can be detected only by extinguishing the components in turn in the manner described on p. 208. A careful search has to be made for lines which are modified sufficiently to show the effect, and, when they are found, the measurements must be made with the utmost care before reliable conclusions can be drawn. In spite of these difficulties, the workers at Mount Wilson have not only detected a solar magnetic field, but they have found its variation with height in the Sun's atmosphere, relying on flash spectrum observations of the levels at which the various affected lines originate. It appears that the strength of the field changes from fifty to ten gausses (Earth's field = 0.47 gauss at London), for an ascent of 125 miles in the lower regions of the atmosphere. This is a very rapid decrease,

and, like the origin of the field itself, is shrouded in mystery.

It will now, it is hoped, be clear how the study of the Sun may be used to amplify our knowledge of the stars in general, by making possible an analysis of those regions—the atmosphere and the interior—which we already know to belong to the great majority of stars. There remains the possibility that the Sun, on account of its nearness, will reveal other regions which, in the stars, are hidden from our knowledge altogether. As a matter of fact, there are two types of solar appendage of this kind—the *prominences* and the *corona*—to which we must now give a little consideration. They are both found outside the Sun's atmosphere, though not discontinuous with it, and we owe to them some of the most interesting problems of solar physics. Of course, in one sense the whole continuous region outside the Sun's interior, including prominences and corona, may be called the "atmosphere." The prominences, in particular, are certainly atmospheric in origin, and, in a study of the Sun for its own sake, would naturally be considered as a part of the atmosphere. But, from our point of view, the Sun is of interest only in so far as it is a type of other stars, and anything in it that is not found in other stars must be considered apart, as a separate existence. The name "atmosphere" is reserved for the place of origin of the absorption lines in the spectrum. The prominences are, in general, not that, and have therefore been omitted in our analysis of the Sun's atmosphere.

Prominences were first observed as red flames, apparently projecting from the edge of the dark Moon, during a total solar eclipse. It was long, indeed, before it was definitely established that they belonged to the Sun and not to the Moon, for it was impossible, for more than 150 years after they were discovered, to see them when the Sun was not eclipsed. The reason for this is that their light, like the light of the stars, is put out by the diffusion of general sunlight in our atmosphere. In 1868, however, Janssen and Lockyer independently devised a spectroscopic method of seeing the prominences at any time in full sunlight, if they were at the Sun's limb. By employing a spectroscope giving large dispersion, the diffused sunlight was greatly spread out and weakened, while the emission lines of the prominences were merely separated from one another without loss of intensity.

PLATE XXIX.



(*a*)

(*b*)

The Solar Corona.

(*a*) 17th May, 1882 (Sunspot Maximum.)
(*b*) 25th May, 1900 (Sunspot Minimum.)

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When the emission lines become visible, it is necessary only to widen the slit in order to see the whole of a prominence in the light of any one of the lines. In this way it was proved, for the first time, that the prominences were masses of glowing gas—mainly hydrogen, helium, and ionised calcium. With the spectroheliograph, Hale has been able to study prominences at any part of the Sun's disc, and so to follow the history of each prominence so long as it is on our side of the Sun.

Just as sunspots are believed to be due to sudden uprushes of gas from the Sun's interior, so prominences may be described as sudden uprushes of the atmospheric gases. They assume a large variety of forms (see Plate XXVIII), whose changes may be so rapid as to be visible from the Earth in a few minutes. This implies velocities of the moving gases of hundreds of miles per second. Evershed has divided solar prominences broadly into two groups, containing respectively the long-lived and the short-lived ones. The former are generally massive, and do not show any marked connection with spots. After a career lasting, perhaps, for several rotations of the Sun, they frequently disperse, apparently into space, with great suddenness. The short-lived prominences are usually found in association with young spots, but the precise connection between the prominences and the spots is not yet settled.

Father Fényi has made the latest contribution to the study of prominences, as the result of thirty-two years of observation with the same telescope. He finds that prominences, like sunspots, show an eleven-year period, which characterises both the numbers and the total areas of the prominences. The times of maximum and minimum almost coincide with those of the spots. When the solar activity is at its minimum, the prominences occur mostly at about latitude 50° ; there are very few at the equator, and none at the poles. As the period progresses, the region of greatest prominence activity moves towards the poles, where it remains only for a short time at maximum. There can be no doubt that prominences and spots have very much in common, and are perhaps fundamentally due to the same cause.

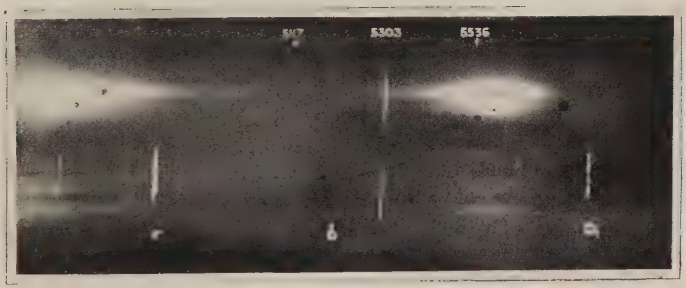
It appears that the corona (Plate XXIX), also takes part in the general solar activity which gives rise to the prominences and the spots. The corona is still visible only during a total solar eclipse, and, but for the fortunate circumstance that

we have a Moon whose apparent diameter is sometimes just a trifle greater than that of the Sun, we should still be ignorant of its existence. When the last remaining crescent of the photosphere is just obscured by the black disc of the Moon, the corona suddenly bursts into sight—a beautiful, pearly white halo, which is perhaps the most impressive feature of a solar eclipse. The form of the corona varies considerably, in a manner which seems to be closely associated with the sunspot and prominence period. At maximum, the corona appears as an approximately symmetrical glow round the Sun ; but at minimum long equatorial streamers are seen, accompanied by tufts of light at the poles. Plate XXIX shows the two types of corona.

The connection between the form of the corona and the sunspot period implies, of course, that the corona must bear at least an apparent relation to the prominences. At sunspot maximum, when the prominences occur at the poles, the corona has its symmetrical form, whilst the coronal streamers and tufts are contemporary with the prevalence of prominences in the lower latitudes. Father Fényi, in agreement with a previous suggestion of Major Lockyer, considers that this points to the prominences as the origin of the coronal rays. Whether this be so or not, the ultimate cause of all these periodic solar phenomena is almost certainly the same.

The spectrum of the corona is of extreme interest. There is a faint continuous spectrum, superposed on which are a number of bright lines (see Plate XXX., upper part ; the lower part arises from a high prominence). Part of the continuous spectrum arises from photospheric light reflected by the small particles of which we assume the corona to be composed. This is shown by the existence of polarisation phenomena characteristic of reflected light. But the bright lines must result from actual emission occurring in the corona itself, and the interesting thing about them is that they have never been produced in the laboratory, so that we do not know from what substance they arise. It has been assumed that they belong to an element as yet unknown on the Earth, to which the name *coronium* has been given : it may be, however, that some known substance or substances could be made to produce them under special conditions of excitation. Nicholson has pointed out a simple mathematical relation between the wave-lengths of the principal lines, and when, in 1914, a red line at $\lambda 6374$ was seen for the first time in the corona

PLATE XXX.



Spectrum of the Solar Corona.

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spectrum, its wave-length was found to fit in with this relation with remarkable accuracy. This suggests that the lines have a common chemical and physical origin, but, in apparent contradiction of this conclusion, it must be remarked that whereas, in previous eclipses, the most prominent coronal line was a green line at λ5303, the appearance of the red line in 1914 was attended by a weakening of the green line almost to invisibility. The solution of the problem will probably come from laboratory experiments rather than from mathematical discussions.

The Sun has taught us many things which, in the next chapter, we shall apply to the study of the stars. But with regard to the causes of its phenomena it is silent: it guards its secret well. We have seen enough to show us that, within the Sun, some fundamental process is taking place which results in the periodic changes we witness: what that process is, remains unknown. There is one problem, the solution of which would go far to give substance to the outline of cosmic processes which we built up in the last section. Given a large, hot, rotating body, to deduce a discontinuity of structure near the surface, a magnetic field, and a periodically changing process giving rise to sunspots, prominences, and corona. It may be that the magnetic field must be included among the data: whether that be so or not, the problem is so far unsolved. Perhaps, with our present knowledge, it is insoluble. Under the special conditions existing in the Sun and the stars, new forces might come into play, of which we have had no experience. But that is a possibility we are loth to admit before a final demonstration has been given that known forces cannot account for the facts. The history of Science is full of new deductions from familiar premises: it is not yet clear that further deductions are impossible.

Meanwhile, we can consider the stars in the light of the knowledge we do possess. We cannot, of course, conclude indiscriminately that everything we have found in the Sun is to be attributed to the stars generally: that would be too big an assumption to make. But we can apply to the study of the stars certain principles which we have learnt from our study of the Sun, and thereby attack problems which otherwise might not come within our reach.

CHAPTER XV

THE STARS

THE study of the Sun—at any rate, so far as it may be regarded as throwing light on the constitution of the stars—has been most successful in its application to the atmosphere, or chromosphere. We know from spectroscopic evidence that the atmosphere of the Sun as a whole is typical of the atmospheres of other dwarf *G* type (*dG*) stars, and we have been able to analyse and interpret the phenomena of the solar atmosphere with considerable success. The interior of the Sun may equally well be regarded as typical, but we have not been able to examine it in nearly so much detail: its great secret is still intact. Prominences and the corona have been detected, and it seems likely that other stars will resemble the Sun in possessing these members, but there are no definite grounds for asserting so. They serve us best as reserve phenomena, which may reasonably be called in to account for any observations of the stars which, without them, are difficult or impossible of explanation. It is, then, to the atmosphere of a star that we must look for the most extensive field of application of solar discoveries, and we therefore give it first place in our story.

Stellar Atmospheres.—We may suppose that the atmosphere of a *dG* star—and, with but slight modification, that of a giant *G* star also—is essentially similar to the atmosphere of the Sun. But we need not stop there. We can apply the principles of Saha's theory to stars of other types as well, and explain, if not the details of every spectrum, at least the main features of the change of spectral type along the Harvard sequence. The necessary data are the changes of temperature and pressure in stellar atmospheres as we go from type to type, and the general character of these changes we know. There can be little doubt that the atmospheric temperatures decrease from type *B* to type *M*. It is true that our measurements are necessarily confined to photospheric temperatures, but it is almost certain that the

temperatures of the atmospheres are in the same order as those of the photospheres. The pressures we may take to be in the order of the densities, for although the latter property, as it is measured, represents the *average* density of a star, it is not likely that the distribution of density throughout the star will change greatly during the course of evolution. From *B* to *M* along the dwarf branch, then, the temperatures fall and the pressures rise, while along the giant branch the temperatures fall and the pressures fall also.

We can see at once that, in the main, the theory agrees with the facts. In Chapter V., it was pointed out that "with advancing spectral type, there is a continuous diminution in the vigour of the light-producing stimulus." The theory identifies the light-producing stimulus with the atmospheric temperature and pressure, and a glance at Saha's formula (p. 198), will show that, of these, the more effective is the temperature. Consequently, the order of spectral type should follow the order of temperature more closely than it should follow the order of pressure—as, in fact, it does. The influence of pressure, however, is by no means negligible, and it is a little surprising at first that the spectra of giant and dwarf stars, of the same spectral type, should so resemble one another as to have been grouped together in the original Harvard sequence. Let us examine this point a little more closely.

We have seen that, if we compare a giant and a dwarf star of the same spectral type, the photosphere of the dwarf will, in general, be found to be hotter than that of the giant. Presumably, then, the atmosphere also will be hotter in the dwarf, and density measurements indicate that the pressure there will be greater. We have, therefore, in the dwarf, a higher temperature, tending to raise it above the giant in the Harvard scale, and a higher pressure, tending to bring it below. The temperature difference we may reasonably suppose to be considerably less than the pressure difference. Bearing in mind the fact that temperature is a more effective stimulus than defect of pressure, it will be clear that the two influences will approach equality, and will leave the dwarf with a spectrum very similar to that of the giant.

The spectra will not be exactly the same, however, because different elements respond differently to the respective stimuli. For example, the degrees of ionisation of elements will change by different amounts for given changes of

temperature and pressure, because the ionisation potentials of the elements are not the same. This problem is under investigation by Russell, but definite conclusions are limited at present by our ignorance of the ionisation potentials of a number of elements which contribute freely to stellar spectra. We can see, however, in a general way, that the difference between the spectra of giants and dwarfs of the same type will show itself mainly in a difference of intensity of neutral and enhanced lines—the very criterion arrived at by Adams and Kohlschutter in their investigation of the absolute magnitudes of stars. There is every likelihood that before long the spectroscopic determination of absolute magnitude will be established on firm theoretical grounds.

At the same time, it must be recognised that the details of stellar spectra can hardly be expected to follow from the present form of Saha's theory. So long as temperature and pressure are the only causes of ionisation that are taken into account, the consequences of the theory can, at best, but approximate to the truth. The changing states of an atom must, to a considerable extent, be due to absorption of radiation from the photosphere, and there may be other agencies which play some part in the matter. For example, Hemsalech and de Gramont have shown that an electric field is sometimes more powerful than high temperature in promoting ionisation—though, if we can be guided by investigations of the Sun, there is no appreciable electric field in the atmosphere of a star. Again, Deslandres has given evidence for the emission, by the interiors of stars, of very penetrating radiations, which are known to have the power of ionising gases. The centre of a giant star is probably hotter, on the average, than the centre of a dwarf star, because of the greater average mass of the giants, and these radiations—which, by their penetrating power, are able to reach from the centre to the atmosphere, and even outside the star altogether—are therefore more powerful in the giant. If the radiations actually exist, they afford, not only another factor tending to increase ionisation, but an alternative, or supplementary, explanation of the spectroscopic criterion of absolute magnitude.

Saha has calculated the atmospheric temperatures of stars of different spectral types from the relative strengths of the spectral lines, according to the equation given by his theory. His results for most types appear to be too high—higher for

some than the measured photospheric temperatures. This, of course, would be expected from the neglect of subsidiary causes of ionisation : the temperature has to bear a greater responsibility than it can carry, and accordingly comes out too high. If we had an independent method of measuring the temperatures of stellar atmospheres, we should be able to estimate how much of the ionisation, in the stars of different types, is due to the subsidiary causes. At present we cannot do this with any confidence of accuracy.

But while there are many details to be settled, we can say that we have traced the main causes of the spectral variations among stars. Laboratory experiments have shown the conditions necessary to produce the different types of spectra : the study of the Sun's chromosphere has shown how those conditions are distributed in stellar atmospheres. Together the two lines of investigation have led to the explanation of the Harvard sequence. We must now pass in mind through the vanquished atmospheres into the stellar interiors, and face the more difficult task that awaits us there.[1]

The Interiors of Stars.—It will at once be obvious that, to confront the interior of a star with any hope of success, we must be armed with new weapons. Laboratory experiments help us but little—at any rate, in a direct way : we cannot reproduce the interior of a star and examine it at leisure. Solar research apparently is equally futile : it has been tried and found wanting. Beyond a few symptoms of its activity, such as sunspots and their attendant phenomena, the interior of the Sun reveals nothing to our observation. We must look for other methods of attack if our quest is to succeed.

It appears that, in physical theory, a weapon has been found to which the problem is vulnerable. Throwing aside direct observation and laboratory experiments *ad hoc*, we face the interior of a star with a bold statement of what it is, and challenge it to show, by its actions, that we are wrong. Confident that it must be known by its fruits, we compare its characteristics with those which our statement requires, and read in the comparison the truth or falsity of our

[1] Saha's work has lately been developed considerably by R. H. Fowler and E. A. Milne. They have shown how the theory can be applied to the determination of atmospheric temperatures with more precision than that attained by Saha, and also how approximate values of the pressures in stellar atmospheres can be deduced.

accusation. The attack along these lines, which has been markedly successful, has been led by Eddington.

Whatever other forces may, in various circumstances, operate in a material body, we have no reason to doubt that gravitation is always at work. We can, therefore, begin by supposing that the material of a star is tending to move towards the centre—in other words, that the star is contracting—owing to the mutual gravitation of its parts. If this tendency were not resisted, the star would, in a very short time (cosmologically speaking), degenerate into a cold, solid body. The fact that it does not do so shows that there must be resisting forces, and, further, the observed constancy of the various characteristics of stars shows that these forces must be almost exactly equal to the forces of contraction. If Russell's theory of evolution is true, the latter must slightly preponderate, but their excess is so small that, in considering the state of a star at a given time, it may be ignored.

The question arises: what are the forces resisting the contraction of a star? There is, first of all, the elasticity of the internal parts. These are pressed downwards by the weight of the upper layers, and exert a reaction, which tends to support the superincumbent mass. But that is not all. A star is intensely hot, and its atoms are therefore radiating energy at a very rapid rate. This radiation takes place throughout the star, the more intensely the hotter the radiating matter, so that the interatomic regions of the star must always be filled with radiation—etherial energy of the same nature as light, but of much smaller wave-length because of the extremely high temperatures responsible for it. Now radiation exerts a pressure, extremely small in ordinary terrestrial circumstances, but capable of becoming very large when the radiating body is hot enough. This pressure of radiation was predicted by Clerk-Maxwell many years ago, and was afterwards demonstrated experimentally by Lebedew and by Nichols and Hull. The pressure is proportional to the fourth power of the absolute temperature of the radiator, and therefore increases at a very rapid rate as the temperature rises. We must assume, then, that the gravitational weight of the outer shells of a star is resisted by radiation pressure as well as by the material elasticity of the stellar core.

Are there any other resisting forces? There may be; in fact, we can say, almost with certainty, that there are—

centrifugal force, for example. There may also be consolidating forces other than gravitation. But it is not our purpose now to consider every influence that may be at work inside a star : we are making, as it were, a guess at the most potent, with the object of working out the consequences of our guess, and comparing them with the observed characteristics of the stars. An intelligent economy is as important in physical theory as in political practice, and for the present it is not wise to introduce factors which complicate our investigations without making a proportionate return.

Let us suppose, then, that, at any level in the interior of a star, there is a gravitational pressure of the outer parts towards the centre, which is balanced by the material elasticity and etherial radiation pressure of the inner parts. This is a clear statement, but it is not sufficient to give us a definite idea of the conditions inside the star, because it concerns only one half of the problem. Not only is the material part of the star in approximate equilibrium ; the energy also is in like case, and we must consider the equilibrium of the energy of the star as well as that of its matter. And just as we had to admit two kinds of resisting force, one arising from radiation, and the other from material pressure, so we must admit two kinds of energy, the first appearing as radiation and forming the chief agency of *distribution* of energy in the star, and the second as heat, or motion of the material particles. We have to express the conditions that the energy in each of these forms is in an approximately steady state.

The energy of a star in equilibrium must satisfy the condition that any given portion of the star must receive and lose energy at the same rate : this condition, in fact, is inherent in the idea of equilibrium. Energy is transmitted from one place to another mainly in three ways—by *conduction*, the handing-on of energy from layer to layer of a material body ; by *convection*, the bodily movement of matter carrying its energy with it ; and by *radiation*, the transmission of energy by etherial waves across empty space. In special forms, energy may travel by other means. A complete statement of the equilibrium condition of energy would have to include all these possible modes of transfer. But, as we have said, we are not now seeking a complete statement : we want to take into account only the prevailing influences. We assume, then, on reasonable grounds, that radiation is by far the most favoured vehicle of energy in a star, and we

express the conditions of energy transfer by saying that the star is in a state of *radiative equilibrium*; *i.e.* that the total radiation received by any interior region of the star in a given time is equal to the radiation which that region discharges to its surroundings.

The idea of radiative equilibrium is of fairly recent birth. It is due mainly to Schwarzschild, though Eddington was the first to introduce it into the discussion of the problems presented by a star's interior. There are three things that should be said about it before we pass on to consider the heat energy in a star. In the first place, though any internal portion of the star receives and emits energy all round its boundary, yet, on account of the symmetry of shape and temperature distribution in the star, we can think quite legitimately of a net flow of energy in outward radial directions only. Any internal spherical shell of gas, concentric with the star, will receive a surplus of radiation from the hotter material inside it, and will send a surplus of radiation to the cooler material outside. The radiation which is received is supposed, in the main, to be absorbed—*i.e.* to be used up in moving atomic electrons away from the nuclei—though it is possible that some may be scattered: the radiation which is emitted arises from the return of the electrons towards the nuclei. In the condition of radiative equilibrium, these two resultant quantities of radiation are equal: the rate of removal of electrons is equal to their rate of return. The second point referred to is that, since the stars radiate energy at an enormous rate into space, they must contain some source of energy which compensates them for the loss; otherwise radiative equilibrium would not be possible. What is the character of that source is an old question, which still remains open (see Chap. XIX.). The third point is that, in order to understand the actual condition of a star's interior, we must be able to determine, not only that a given region radiates as much energy as it receives, but also how much energy that may be. The passage of energy through a star is controlled by what is called the *absorption co-efficient*, or *opacity*, of the material of the star. If we can discover the amount of energy absorbed by a given layer, we shall know, by virtue of the condition of radiative equilibrium, the amount of energy which that layer radiates to the next. Unfortunately, since the conditions inside a star are unparalleled on the Earth, we do not know what the opacity of stellar material is. We, therefore, leave

it as an undetermined quantity, and trust to a subsequent comparison of our theory with observation to tell us its value.

Coming now to the question of material, or heat energy, we have to deal with the conditions that, in the main, determine the temperature of a star. It will be obvious, therefore, that we must specify, at the beginning, the type of star we are considering: even the most general investigation must take into account the wide difference in density between giant and dwarf stars. We can assume with confidence that, in the giant stars, the material is in the state known to physicists as that of a "perfect gas"; that is, that the atoms and free electrons are, on the average, so far apart that the amount of space they occupy at any instant is negligible in comparison with the volume of the star which constitutes the field in which they are free to move. Matter in this condition is known to obey a very simple law; namely, that the pressure divided by the product of the density and the absolute temperature is equal to a quantity which depends only on the average mass of the particles—molecules, atoms, electrons, or whatever they may be—of which the matter is composed. In the dwarf stars, the material will no longer be in the perfect gaseous form; it may still be gaseous, but it will be too near the liquid state to obey the laws of an ideal gas. Unfortunately, even under ordinary laboratory conditions, the thermal relations of matter in this state have not been accurately formulated, and an extrapolation from the approximate relations used in physics to those which must obtain under the speculative and widely different conditions inside a dwarf star is bound to be attended by considerable uncertainty. For this reason, much greater progress has so far been made in the exploration of giant stars, and we shall therefore give them most consideration.

In order to apply the gaseous laws to a numerical investigation, we must know the average mass of the ultimate particles of the star. This at once raises a difficulty, because the atoms of the elements have different masses, ranging from 1 to more than 200 units, and we have no means of knowing the chemical composition of a star's interior. But, fortunately, in the intense heat maintained inside a star, all the atoms will be very greatly simplified by the high degree of ionisation which must exist. Each atom will be robbed of most of its electrons, and the quantity we are seeking will be the average mass of a number of sadly mutilated atoms and

the amputated electrons which move about in limited freedom. If ionisation were complete, the average mass would be almost independent of the chemical composition of the star, for the following reason. An atom of any element (except hydrogen), in the normal state, has electrons, of negligible mass, approximately equal in number to half the mass of the nucleus, expressed in terms of the mass of the hydrogen atom as unit: thus, a nucleus of mass $2n$ will normally have about n electrons revolving round it. If, now, all the electrons are detached, there will be $n+1$ particles produced, and, neglecting the masses of the electrons, the average mass of the particles will be $\frac{2n}{n+1}$. This quantity must always lie between 1 and 2, so that, no matter what the star is made of, there is very little uncertainty about the average mass of its constituents. There is reason to believe that ionisation is not complete in a giant star, but very nearly so : some uncertainty is therefore introduced, but Eddington considers that an assumed average mass of 2.8 will probably give results not far from the truth.

To sum up, then, we imagine the interior of a star to be somewhat as follows. There are a number of atomic nuclei, possibly of widely varying masses (but this is uncertain), and a number of free electrons, all moving about at very high speeds and frequently colliding with one another. There is also a quantity of etherial radiation, travelling about from atom to atom, continually absorbed and re-emitted, but, on the whole, working its way slowly out to the surface, and thence to empty space and the spectroscope of the astronomer. The star is maintained in material equilibrium by the opposition of its elasticity and the pressure of its radiation to the gravitational compressing forces ; and in radiative equilibrium by the equivalence of the opacity and the radiating power of each internal layer. In a giant star, there is no appreciable cohesion of the ordinary kind between the ultimate particles, so that the material behaves as an ideal gas ; in a dwarf star, there is such cohesion, but its influence is not exactly calculable. In every star there must be some source of energy which enables the star to maintain its approximate equilibrium in spite of its continuous radiation into space.

This is the hypothesis with which Eddington has confronted the interior of a star : let us see how the star fares under cross-examination. In the first place, we may notice that, according to the hypothesis, there may be an upper limit to

the mass of a star. So long as material elasticity was the only recognised force resisting gravitation, this could not be suspected, for the resistance due to elasticity has the character of a reaction, and can never exceed the force of compression that calls it into play. But radiation pressure is quite different. It depends on the temperature of the star, and there seems to be no reason why a star may not become hot enough for the radiation pressure to exceed the force of compression and so make the star a ready prey for even the smallest disintegrating forces. Eddington has shown that the relative importance of radiation pressure in the resistance to gravitation depends only on the total mass of the star and on the average weight of the ultimate particles, which we have agreed to fix at 2.8 units. If a star has a mass less than 10^{33} grams, radiation pressure contributes less than one-tenth of the resisting force: if it has a mass greater than 10^{35} grams, the contribution is 85 per cent. or more. Within these comparatively narrow limits, the radiation pressure rises from obscurity to sovereignty. It appears, then, that the masses of stars should lie between these limits if the theory is true: there may be, not only an upper limit, but a lower limit also. A star lighter than 10^{33} grams would scarcely become hot enough to shine visibly—its radiation pressure indicates too low a temperature: a star heavier than 10^{35} grams would probably be disrupted, because of the instability arising from the pressure of its radiation. As a matter of fact, the lightest known star has a mass just less than 10^{33} grams, and the heaviest known star has a mass just greater than 10^{35} grams; the great bulk of the stars have masses lying between these values. This is a remarkable result, which goes far to justify the assumptions of the theory. We shall meet later, however (p. 389), with a quite different possible explanation of the limited range of stellar masses.

With regard to absolute magnitude, the requirements of the theory are not quite so definite, because of our ignorance of the opacity of stellar material. If the opacity is constant throughout a star, then it can be shown that in giant stars having the same mass it is inversely proportional to the absolute luminosity. If, therefore, the opacity is the same for all giant stars (perhaps the same assumption as the former one, in a different form), the absolute luminosity must be the same also; that is, the absolute magnitude of a star must remain constant throughout the giant stage. This is quite

in accord with facts, as Russell's diagram shows (Fig. 8).[1] Further, if the foregoing assumptions are valid, we can calculate the opacity. The data enable us to calculate the radiation falling on a stellar layer from the inside ; observation of absolute luminosity shows us the amount of this radiation that escapes : the difference between these two quantities is absorbed, and its amount measures the opacity. It appears that the opacity of stellar material is the same as that of metals in the laboratory for X-rays. It was anticipated that the radiation in a star would be of the same character as X-rays—very short etherial waves—because of the very high temperatures there ; or, put in another way, because the atomic electrons responsible for the radiation are those next to the nuclei, which in the laboratory give X-rays, and not the outermost electrons which give rise to visible radiation. But it was not anticipated that the opacity would be independent of the star, and therefore of the temperature : the temperature determines the wave-length of the radiation, and the opacity of materials under terrestrial conditions varies considerably with the wave-length. It is not necessary, however, to abandon the assumption that the opacity is constant, because the conditions of absorption in a star are vastly different from those of absorption in the laboratory—chiefly owing to the very much greater intensity of the radiation in a star. Eddington states, in fact, that if it is necessary for an electron to hit the atomic nucleus in order to be recaptured, and therefore to absorb radiation, then the opacity should actually be constant for all giant stars, and should be equal to the value which the theory indicates for it. If, then, physical investigation should be able to show that hitting the nucleus is a necessary condition of absorption in stellar circumstances, the constancy of the opacity, according to Eddington's view, would cease to be an assumption, and the observed uniformity of absolute magnitude among giant stars would give whole-hearted support to the theory.

[1] According to the theory it is the absolute *bolometric* magnitude (p. 83) that, under the conditions assumed, remains constant throughout the giant stage. Not much is known as yet of the relation of bolometric magnitudes to the Harvard sequence, but Eddington considers that the absolute visual magnitudes are a reliable substitute for bolometric magnitudes in estimating the general character of the relation. Later refinements of the theory, however, indicate that there should be a slow, continuous decline in brightness of a star while passing through the giant condition—a conclusion to which certain observational evidence also points.

It is interesting to look at the numerical values of a few of the physical constants which play their parts in the interior of a star. Eddington gives the following picture of a giant star, of type *G*, having a mass 1½ times that of the Sun and a mean density of 0.002 grams per cubic centimetre—$\frac{1}{500}$th of the density of water. At the centre, the density would be about $\frac{1}{10}$th of that of water, and the absolute temperature would be 6,590,000°. At about half-way along the radius (which is assumed to be nearly seven times the Sun's) the density would have fallen to about $\frac{1}{400}$—a little greater than the mean density—and the temperature would be about 2,360,000°. At $\frac{6}{7}$ths of the distance outwards, the density would have become as low as $\frac{1}{100000}$, and the temperature 293,000°—much lower than that at the centre, but still far above the measured photospheric temperature. At the boundary of the star (strictly speaking, the base of the atmosphere), and for some small, indeterminate distance below it, the conditions we have postulated cease to hold with sufficient exactness to make a reliable calculation possible. The opacity of the star would be such that a layer about six inches thick, in a region having a density of the same order as that of our own atmosphere, would absorb all the radiation falling on it.

In applying the theory to dwarf stars, some much more doubtful assumptions have to be introduced. In order to make calculation possible, Eddington assumed that the material in a dwarf star obeyed a modification of the perfect gas law which was suggested by van der Waals and has been found to be fairly accurate over a limited range in the laboratory. It was then possible to show that the stars of small mass were unable to rise to the high temperatures of the early type stars—a result, as we have seen, predicted by Russell in connection with his theory of stellar evolution. The general character of Russell's scheme was deduced, and, in fact, Eddington's theory as a whole gives strong support to the idea of evolution which Russell originally derived from observational evidence. Further development of the idea, so far as dwarf stars are concerned, awaits a better knowledge of the properties of materials at high densities, though, even when this is obtained, the validity of extrapolation to stellar conditions will probably be much more questionable than it is with respect to the law of an ideal gas.

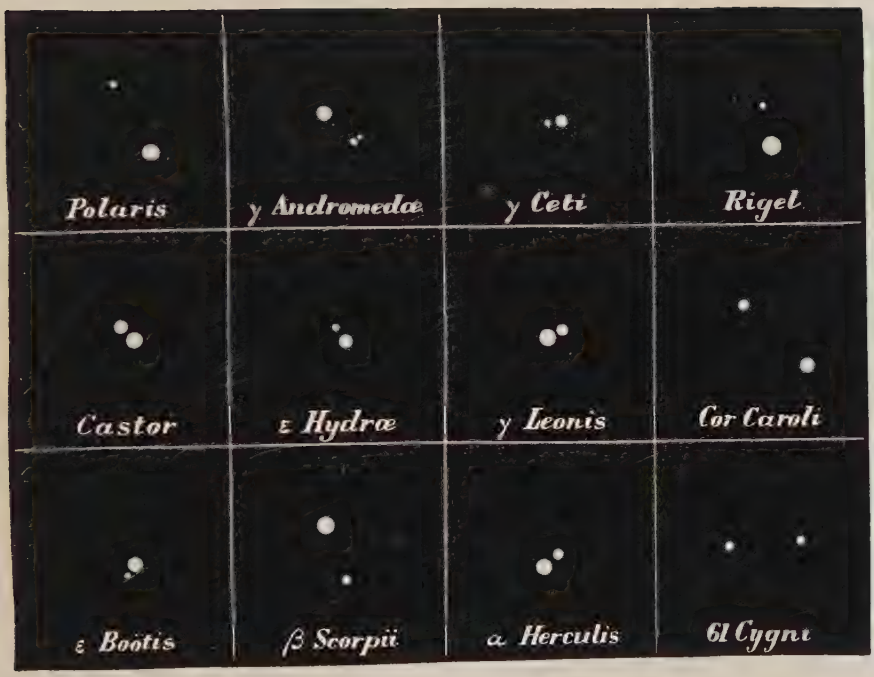
On the whole, however, it appears that the general

character of the processes occurring in a star's interior has been discovered. We have some idea of the influences that are most powerful in determining the mass and output of radiation of a star, and we can interpret the main changes that occur in its evolutionary career. But our knowledge is still only of the broadest kind. We do not know how a star maintains its store of energy—evidently one of its most important functions, the expression of its instinct for self-preservation. Nor do we know how to account for the subtler phenomena, such as solar magnetism and sunspots, which we can hardly suppose to be characteristic only of our own Sun. There is still much to be learnt in this, as in every department of physical astronomy. But a beginning has been made. The way is open now for the investigation of details, and it can be followed with more confidence and a clearer perception of the relative importance of the forces at work than was possible a few years ago. The interior of a star is no longer a complete mystery.

Double Stars.—The study of the Sun, then, has helped us to interpret the phenomena of stellar atmospheres, but has done very little towards the penetration of stellar interiors. We come now to stars whose special characteristic is not found in the Sun at all—the double and multiple stars. It appears that about half of the stars, at least, are double, so that we are dealing not with a freak of Nature but with a normal process which, for some reason, takes place in some stars but not in others. The Sun, perhaps for consolation, has been given a family of planets and comets, and it may be that other apparently lonely stars have families of this kind, though the balance of probability is rather against the frequent occurrence of such systems. Ages ago, it is suggested, a fickle star became attracted by the Sun, moved almost within reach of her, and, by its gravitational influence, begot the bodies which, in the course of time, became the planets. But it passed on, leaving the Sun to rear her brood alone.

The double and multiple stars, unlike our system, consist of bodies comparable in mass and often in luminosity. They are detected in various ways. Some are separated sufficiently to be seen as two points of light, and move under their mutual gravitational attraction quickly enough for the change of their relative positions to be observed. Others appear to be double, but show no orbital motion. With such a star

PLATE XXXI.



Drawings of Visual Binary Stars.

To face page

as these, there is an element of doubt whether it is really a double star that is seen, or two stars at widely different distances from us but almost in the same line of sight—an "optical double" as the pair is called. It is usually possible to settle the matter by measuring the proper motions of the stars. If the proper motions are identical, then the stars may with safety be regarded as the components of a single system whose orbital motion is too slow to be observed. The chance that two stars, having the same proper motion in

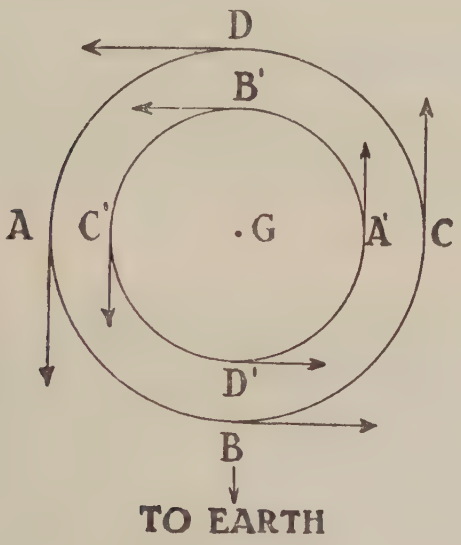


Fig. 18.

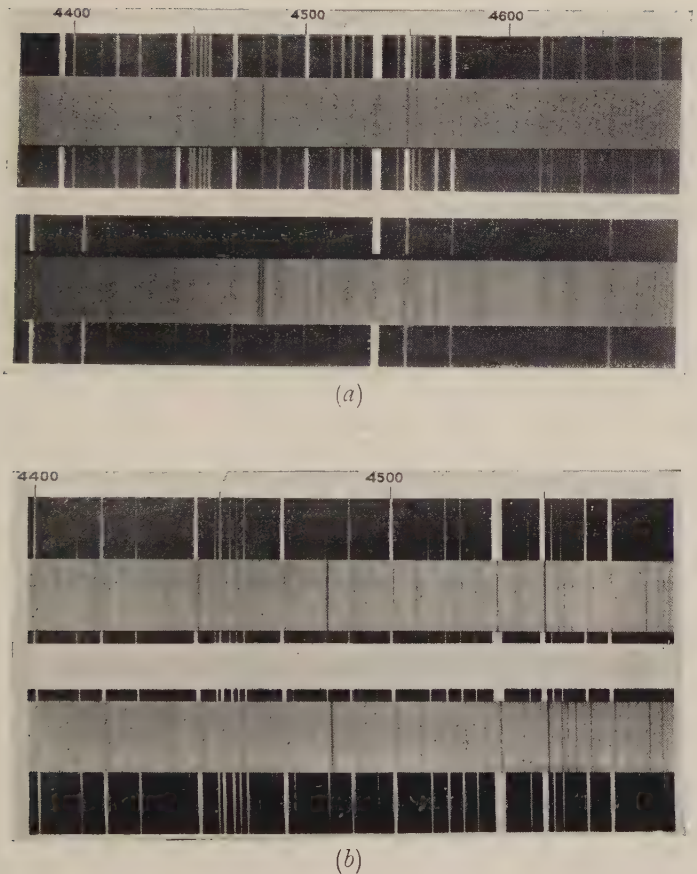
magnitude and direction, should be seen almost exactly at the same point in the sky, and yet not be physically connected, is an extremely small one, and may safely be neglected.

Many stars, which to the telescope appear single, are known to be double by the spectroscopic evidence of their orbital motion. The components of a double star revolve in a certain period round their common centre of gravity, *G*, in the manner indicated in Fig. 18, where corresponding positions of the components are represented by the same letters, dashed and undashed, and the orbits are made circular instead of elliptical, for simplicity. (To meet the general case, in which the plane

of the orbits need not pass through the Earth, we may suppose Fig. 18 to represent the projection of the orbits on a plane through the Earth and the star). Suppose, at first, that both components of the system are bright enough to give a spectrum. Then, when they are at A and A', respectively, the former will be moving directly towards the Earth, and the latter directly away from it. If the speeds are great enough, they will show themselves by a doubling of those lines in the spectrum which are common to both components : the lines from the star at A will be displaced towards the violet, and those from the star at A' towards the red. At B and B', both stars will be moving across the line of sight : their orbital motion will give no Doppler displacement, and the two spectra will be superposed. At C and C', the lines will again be doubled, but the displacements will have been interchanged, for the star which at A was approaching the Earth, is now receding from it, and *vice versa*. On arrival at D and D', the stars will again give spectra without relative displacement. There will thus be a periodic doubling of the spectrum lines of such a system as this, the distance between the components of a line depending only on the wave-length of the line (p. 35) and the speed of orbital motion of the star—not at all on the distance of the system so long as it sends enough light to give its spectrum. If the system, as a whole, has a component of motion in the line of sight, there will be a Doppler displacement of both sets of lines by the same amount in the same direction. This will not affect the periodic doubling of the lines, which depends only on the *orbital* motion : it will show itself by a displacement of the lines, when the component spectra are superposed, relative to the position of terrestrial lines having the same chemical origin. Plate XXXII. (*a*) represents two spectra of Mizar, showing single and double lines respectively. The stellar spectra are each bordered above and below by a comparison spectrum of titanium.

It often happens that only one of the components is bright enough to give a spectrum. When this occurs, the duplicity of the system can still be detected by the oscillation of the single lines. The oscillation will take place, not necessarily about the standard positions of the lines as determined terrestrially, but perhaps about slightly displaced positions which measure the radial velocity of the complete system. Binaries of this type are usually detected during radial velocity determinations. A variable radial velocity suggests

PLATE XXXII.



(*a*)

(*b*)

Spectra of Spectroscopic Binaries.

(*a*) Mizar (ζ Ursæ Majoris)—both components bright.
(*b*) μ Orionis—one component bright.

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that a binary star is being examined, and if the variation is periodic, the suggestion is, as a rule, confirmed. This phenomenon is shown, for example, by the star μ Orionis, which was thereby found to be a binary at the Yerkes Observatory. The two spectra of this star shown in Plate XXXII. (*b*) are each displaced to the right of the comparison spectrum, but the displacement is greater in the lower spectrum than in the upper. The star as a whole is receding from the Earth at a speed of twenty-eight miles per second, and its components revolve round the common centre of gravity in a period of 4.45 days. Stars which are recognised to be double through spectroscopic evidence are known as *spectroscopic binaries.*

It is not often that a spectroscopic binary is also a visual binary, for the conditions favouring the detection of the two types are of opposite kinds. The farther apart the components, the more likely is the star to be a visual binary, because our telescopes will have a better chance of resolving it. But the velocity of orbital motion is greatest when the components are closest together, and it is the binary with high orbital velocity that is most likely to be found spectroscopically. Consequently, the periods of visual binaries are, on the average, much longer than those of spectroscopic binaries. This is fortunate for our catalogues of double stars, but it has disadvantages from the point of view of the completeness of our knowledge of the binary orbits. The orbit of a visual binary which shows relative motion of the components can be determined almost, if not quite, completely. The size of the orbit is obtained only in angular measure (which, however, can be converted into linear measure if the parallax of the system is known), and although the inclination of the plane of the orbit to the line of sight can be found, it cannot generally be decided which of the two planes having this inclination is the one in which the orbital motion actually takes place. Our ignorance on this point can be removed if the star is a spectroscopic binary as well, and it is then not necessary to know the parallax in order to determine the orbit completely. On the other hand, a spectroscopic binary which is not a visual binary leaves us in doubt about the inclination and the size of its orbit. It gives us a relation between these quantities, but it does not reveal them individually. Neither type, therefore, by itself shows the whole circumstances of its motion.

Within the last few years, a great advance in double star astronomy has been made possible by the employment of the interferometer. This instrument enables us effectively to resolve a number of spectroscopic binaries, which appear as single stars to the ordinary telescope, into visual binaries as well. The method of observation is very similar to that which is adopted for the measurement of stellar diameters (p. 131). The line joining the two slits (or subsidiary mirrors) of the interferometer is adjusted to be perpendicular to the line joining the projections on the celestial sphere of the components of the binary, and the interference fringes can then, in general, be seen crossing the stellar image. When the slits are separated, the fringes change in distinctness, and, for a certain distance between the slits, they disappear altogether. This distance measures the angular separation of the components of the star, which, combined with the spectroscopic evidence, enables the orbit to be calculated completely if the parallax of the star is known. The components of the bright spectroscopic binary Capella were found by this method to be 0″.0418 apart in December, 1919.

It is necessary, for the application of this method, that both components of the binary shall give an appreciable amount of light : the nearer they approach equality in brightness, the more accurate will be the measurement. It is not easy to estimate the relative brightness of the components from the spectrum alone. Campbell says that a fourth magnitude companion of a second magnitude star would scarcely contribute to the combined spectrum : it would, however, cause a large diminution of distinctness of the fringes seen with the interferometer. The interference method is therefore of wider application than might at first be thought.

To make the account complete, we must mention one other method by which the duplicity of a few stars has been detected. It will be clear that orbital motion will cause, not only a variable radial velocity—the criterion of a spectroscopic binary—but a variable proper motion as well. This, in general, will be more difficult to observe than the radial variation, but it is well within the range of detection in the nearer stars whose proper motions are large. It was, in fact, in this way that Sirius and Procyon were first found to be binaries with relatively dark companions. In view of the present high standard of radial velocity measurements, it is not likely that proper motions will claim any more discoveries

of this kind, but the method is interesting, if only on account of its past success.

The periods of binary stars cover an enormous range. From periods of a few hours at one end of the scale, we find, at the other end, stars whose components have shown no relative movement in more than a century, and whose periods must be counted in thousands of years. It is possible that all intermediate periods are represented among the stars, but the methods of detection tend to augment the numbers of extreme periods at the expense of those of moderate ones. Remembering that a lengthening of period goes with an increase of distance between the components, it will be understood that there may be large numbers of binaries which go undetected because their separations are too small for visual observation, and too large for the spectroscope to discover. This peculiarity of sclection has to be borne in mind in the classification of binary stars.

On this account, it is best to consider the statistical data in general terms rather than to accept the figures as they stand. It appears that, when due allowance is made for false appearances, two real correlations remain. First, the longer the period of a binary, the more eccentric,[1] on the average, is the orbit ; and, second, the longer period binaries are most prevalent among the later spectral types, and the shorter period binaries among the earlier spectral types. The second correlation is not so pronounced as the first, but there seems to be no doubt about its reality. The spectroscopic binaries, whose periods range from about three days to three or four years, are mainly of types *B* and *A*, while the visual binaries, with periods of thirty years and upwards, have scarcely a representative in class *B*, but show a strong preference for classes *F* and *G*. We have dealt in Chap. XII. with the explanation of these facts in terms of stellar evolution. It appears, further, that spectroscopic binaries as a whole form, in some way, a different class of body from visual binaries. Each class shows the period-eccentricity correlation strongly, but the correlation is not continuous from one class to the other. Thus, there is a wide gap in period between the most rapid visual binaries and the slowest spectroscopic binaries, but the corresponding gap in eccentricity is relatively

[1] The *eccentricity* of an orbit measures its departure from the circular form. An orbit with small eccentricity would be nearly a circle, while one with large eccentricity would be a long, narrow ellipse.

very small. We shall see, in a moment, a possible explanation of this fact.

The method of origin of binary stars raises an interesting problem, which has excited a great deal of attention. There are three distinct possibilities, each of which probably accounts for some binaries, though they are by no means equally productive. In the first place, a binary star may originate in the division, or *fission*, of a single star which, by its contraction, acquires such a high rotational velocity as to become unstable. Secondly, in a contracting mass of matter, the condensation might not take place uniformly. If, for some reason, two condensed spots or nuclei are formed in the mass, further condensation might take place around each of them, causing the mass ultimately to separate into two distinct stars. Thirdly, a binary might be the result of capture—by gravitational attraction during an abnormally close approach —of one star by another which originally was quite independent. The relative frequency, in the actual stellar universe, of these three types of origin has been subjected to an examination as complete as the data will allow.

Unfortunately, no light is thrown on the problem by a study of the relative masses or luminosities of the components of binaries. It might be thought that these quantities would vary, on the average, according to the types of origin of the binaries, and so probably they would. But when the influence of selection is taken into account, it is found that no conclusions can be drawn from the present data. A more promising line of inquiry was followed by Russell about thirteen years ago. He studied the dynamical consequences of the breaking up of a mass by fission owing to the cause mentioned above, and compared the results of his calculations with observation. He found considerable agreement ; in particular, multiple stars, say triple ones, were just as they should be on this hypothesis of their origin—binaries with relatively distant companions. The method of calculation, however, was such that the results applied equally well to the " separate nuclei " type of origin. If, from the agreement of deductions with facts, we are to conclude that the hypothesis leading to the deductions represents what has actually taken place, then it is an open question whether fission or condensation round separate nuclei, or both, must be accounted the source of the binary stars.

A little later, Jeans showed how a partial discrimination

between the possibilities could be made. He proved that rotational fission could not take place if the density of a star was lower than a certain value, and many giant binaries were known to have densities below this value. The limit of density, in fact, was that at which a star, as a whole, ceases to behave as a perfect gas—and therefore, according to the theories of Russell and Eddington, it coincided approximately with the density of a star at its highest temperature, at the point at which it ceases to be a giant and begins its descent along the dwarf branch. No giant star, then, can break into two by rotational fission. But there was no such limitation on the possibility of condensation round separate nuclei. Division in that way could take place at much lower densities. Consequently, giant binaries, or binaries and multiples that may be assumed to have come into existence during the giant stage of a star, must represent, according to Jeans, " survivals of separate nuclei in the original nebula."

There is, however, a defect in the above argument, because, in comparing existing orbits with the orbits which would result from certain hypotheses, it is tacitly assumed that the existing orbits have not been modified by external forces at any time during their existence. In our ignorance of the past history of the Universe, we have no right to assume this. Jeans therefore approached the problem in a new way. He calculated what proportion of the existing binary orbits should have periods and eccentricities of various values, on the assumption that the orbits might have arisen in any possible way at all, and that they had been knocked about and modified to an indefinite extent since their formation. If the actual orbits were then found to show a distribution, with respect to period and eccentricity, agreeing with that obtained from the calculation, the result would mean that the actual orbits had been so disturbed as to give no evidence with regard to their origin ; their forms would be due entirely to subsequent external influences. If, on the other hand, the actual distribution, when compared with the calculated one, showed a marked excess of periods or eccentricities of a certain range of value, then that excess would indicate that the influence of the method of origin was still to be seen in the orbits, and the values of the period and eccentricity which were in excess would indicate what that method of origin was.

Proceeding in this way, Jeans found that there was a

general tendency for the actual distribution to conform to the calculated one, showing that external forces had been at work on the orbits of binaries. But there were two conspicuous differences. Among the actual binaries there was a marked excess of two types of orbit—first, orbits of low eccentricity and short period, almost all of them associated with spectroscopic binaries; and, second, orbits of moderate eccentricity and period between 30 and 500 years, all belonging to visual binaries. The obvious inference was that there were two preponderating methods of formation of binary stars, which gave rise to differing types of orbit, and that the characteristics of the two types had not been completely destroyed by extraneous forces operating after the orbits had been formed. The problem was then to determine, from the character of the prevailing orbits, what were the two processes by which, respectively, they had come into being.

The first class of orbit would be the natural result of rotational fission. A binary formed in this way would have a short period, for its components would be almost in contact, and the orbits would be almost circular. If this is the true origin of this class, they must have been formed somewhere about the end of the giant stage of the corresponding stars—at any rate, not earlier—and the periods and eccentricities must have been increasing continuously after the formation of the orbits. In other words, spectroscopic binaries should be nearly all dwarfs, and the periods and eccentricities of their orbits should increase with advancing spectral type. This, in fact, is just what we find. Generally speaking, then, we conclude that spectroscopic binaries have been formed by fission.

The second type of orbit could not have originated in this way. External forces must have tended, on the average, to shorten the period, so that the original periods must have been even longer than the present ones. For fission to take place subject to this condition, the density of the parent mass would have to have been of the order of 10^{-12}—far too low for fission to be possible at all. This objection, however, does not apply to the mode of formation by condensation round separate nuclei, and it is believed that this is the way in which the majority of visual binaries have been formed. There seems to be no likelihood that any appreciable number of binaries arose in the third possible way—by capture of one

star by another. Doubtless such a thing must have happened occasionally in the history of the stars, but in the actual distribution of orbits its effect is not observed.

Speaking in general terms, then, we may say that spectroscopic binaries were formed by fission and visual binaries by condensation round separate nuclei. The influence of selection, as we have seen, must operate strongly on the data for binary stars, and it would, in fact, tend to give, by itself, the results we have assumed to be real. It is necessary, therefore, to check the conclusions by making as representative a selection as possible. Jeans has done this by considering the 20 brightest binaries alone. These may be assumed to be relatively near to us, and therefore open to detection whatever may be the distance between their components. A few of them are multiple stars, so that Jeans had 24 orbits altogether to consider. He found that 13 of these, all of which corresponded to spectroscopic binaries, had periods not greater than 104 days: 10 (5 spectroscopic, 5 visual) had periods not less than 1515 days: and the remaining one, which had a period of 684 days, was difficult of interpretation for reasons other than those we are now considering, and could be regarded as an exception.

The interval between 104 and 1515 days is too pronounced to be accidental, and it was regarded as indicating a division between the two classes of binaries formed, respectively, by rotational fission and by condensation round separate nuclei. Of the 13 short period orbits, 9 were found in dwarf, and 4 in giant stars. According to the conclusions reached above, giant stars should not divide by fission, so that these four stars demand attention. Jeans maintains that it is doubtful whether they are binaries or not. The evidence is entirely spectroscopic, and, as we know, there are many causes of displacement of spectrum lines. There is, in fact, a large class of stars—the Cepheid variables, which we shall examine presently—which show the spectroscopic criterion for duplicity perfectly, and yet, on strong grounds, are believed by many not to be binaries at all. One of the reasons for this belief is that, whereas, with most binaries, the *mass-function* (a quantity depending on the ratio of the masses of the components and the inclination of the orbit to the line of sight) is not far from unity, its value (calculated on the supposition that the stars are binaries) is very low in the Cepheid variables—of quite a different order from the usual one. Now the mass-functions

for the four stars we are considering are very low also. The exact value for one of them is not known, but for the other three the highest value is 0.0001. This is a striking anomaly, and, taken in conjunction with the dynamical difficulty we have referred to, is thought by Jeans to cast great doubt on the reality of the supposed binary character of these stars. Moreover, among the stars as a whole, almost all the giants which, according to the above principles, would be classed as binaries formed by rotational fission, are found to show the characteristics of Cepheid variables. There are only three doubtful cases, for which an explanation of some other kind may be found.

It may be questioned whether the evidence for denying the binary character of the exceptional stars is sufficiently strong. In the Cepheid variables, as we shall see, there are several reasons for doing so which do not apply to the stars we have just mentioned. The evidence on the whole seems to be so markedly in favour of Jeans's hypothesis that it is hardly necessary forcibly to capture unwilling witnesses in order to make it convincing. It must be remembered that Jeans has taken only gravitational forces into account in his calculations. That there are other forces which influence the form of a star, we well know. There is radiation pressure, for instance, and the unknown causes of sunspots and of Cepheid variation. It seems more reasonable to regard the facts as showing that gravitation has the paramount influence in the formation of a double star, but that there may be other influences which occasionally get the upper hand. However that may be, it is probable that, on the whole, spectroscopic and visual binaries, so far as origin is concerned, represent two different types of star.

Jeans has traced mathematically the successive stages in the fission of a star. A globular mass of gas rotating on an axis can be in equilibrium only when its shape departs from that of a perfect sphere. For slow rotation a spheroid is a stable shape, the shorter axis being the axis of rotation. The equatorial bulge of the Earth, for example, is regarded as an adjustment of shape necessary to keep the Earth in equilibrium in spite of its rotation. As the gaseous mass contracts by its own gravitation, the speed of rotation must increase, and the equatorial axis must get larger and larger in comparison with the polar axis. At a certain speed the spheroidal form is no longer able to maintain equilibrium, and the star

becomes ellipsoidal; *i.e.* it has three unequal axes. With further contraction and consequent speeding-up of the rotation, equilibrium becomes impossible even in this form. The star has now no alternative to breaking up. The first stage in the disruption is the formation of a constriction, such as that shown in Fig. 19 (*a*)—the axis of rotation is parallel to the edge of the page. This becomes more and more pronounced, until at last the star breaks into two (Fig. 19 (*c*)), which revolve round their mutual centre of gravity in orbits which tidal forces tend to make larger and larger up to a certain limit. A star is relatively only a very short time in the conditions represented by Fig. 19 (*a*) and (*b*), so that we should not expect to find many such stars at any one time. There are three known stars, however, which are possibly just about to divide into two: in *RR* Centauri, the calculated radii of the components are together greater than the radius of the orbit,

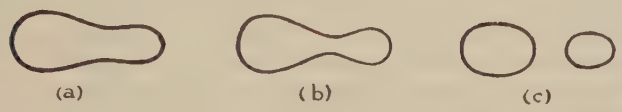


Fig. 19.

Stages in the formation of a Binary Star by Fission.

which means that the stars overlap; and the components of *RT* Sculptoris and *X* Carinæ are either in this condition or else have only just separated. Further, stars in which the components are probably actually in contact are known—for example, β Lyræ. There is, in fact, as much evidence for the reality of Jeans's calculated course of events as we should expect to find.

A recent investigation by Leonard[1] of eighty visual binaries, including both giants and dwarfs, gives an extremely interesting corroboration of Russell's theory of stellar evolution. Leonard examined the spectral types and absolute magnitudes of the components of the binaries, and, for some of the stars, he was able also to calculate the masses. He found that, in general, the fainter component was in the more advanced stage of development according to Russell's criterion. In the light of what we now know about the origin of binary stars, we must conclude that the components once formed a

[1] A similar investigation was previously made by Doig.

single mass, so that, on separation, they must have been in the same physical state. Leonard's result, then, implies that the fainter component must run through its life at a greater rate than the brighter. This would occur, according to the theories of Russell and Eddington, if the fainter component were the less massive : if that were so, then its relative faintness and advanced development would both be expected. Leonard has been able to show, in agreement with former investigators, that, in general, the fainter component *is* the less massive. Moreover, there should be a relation between the difference in brightness and the actual period of life of the components, because, during the giant stage, the difference in magnitude should arise only from the difference in mass, whereas, during the dwarf stage, there should be a further difference in brightness owing to the difference in spectral type. Thus, if the components are both giants, of types *G* and *F*, respectively, their difference in magnitude should be small : if, however, they are *F* and *G* type dwarfs, then they should differ much more in brightness. Leonard showed that this was quite in agreement with fact, and, further, that the greater the interval between the effective " ages " of the components, the greater was the difference in magnitude. It would be difficult to find a more convincing argument for the truth of Russell's theory than this.

Another interesting result obtained by Leonard, which appears not to have been foreseen, is that, as the sum of the masses of the components increases, the disparity in development gets less and less. The evidence for this conclusion is derived from thirteen dwarf stars. Apparently it means that, the greater the parent mass, the better chance there is that the nuclei will share the available material equally. If the result is corroborated, it might provide a useful clue to the more complete understanding of the formation of visual binaries.

Eclipsing Binary Stars.—A binary star, whether spectroscopic or visual, has a particular interest for us when the plane of its orbit passes through or near the Earth. When this happens there are periodic eclipses (total, partial, or annular) of one component by the other. There is, consequently, a periodic variation of the apparent brightness of the star, for, at regular intervals, the light of each of the components is completely or partially obscured, and the star shows a

temporary decline in brilliance. A star whose apparent magnitude fluctuates, from this or any other cause, so rapidly as to be obviously changeable, is called a *variable* star: several thousands of such bodies are known. The word "variable" applies, in the first instance, to the apparent magnitude: it may, or may not, be applicable to other stellar characteristics. A variable star is not the opposite of a fixed star: the latter term is used of all the stars, except

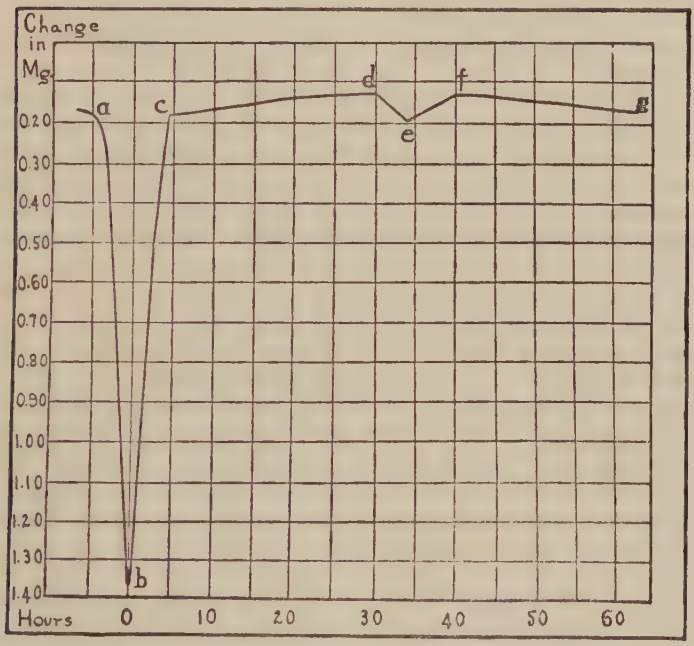


Fig. 20—Light-Curve of Algol (β Persei).

the Sun, as a tribute to the extreme smallness of their proper motions. Variable stars form a very important class of body, whose characteristics we must now examine.

Beginning with eclipsing variables, we can say at once that it is a pure accident that these stars are variables at all. They differ in no manner from invariable binaries except in the orientation of their orbit-planes with respect to the Earth—a peculiarity that can have no intrinsic significance. Nevertheless, from our point of view it is of very great

importance, for it enables us to learn a great deal about the stars which would otherwise remain unknown. The circumstances of the variability are usually expressed by a *light-curve*, which is a curve showing how the apparent magnitude of the star changes throughout the period of the variation. The apparent magnitude of a star which is suspected of being a variable is measured at frequent intervals, and the results are plotted until, if the variation is confirmed, the sequence of changes throughout a complete period is established. From the character of the sequence, the type of variable and, if possible, the origin of the variation are deduced.

Fig. 20 shows the light-curve of Algol (β Persei), a typical eclipsing variable.[1] It will be noticed that there are two positions (*b* and *e*), one much lower than the other, at which the brightness reaches a sharp minimum. These positions correspond to the greatest phases of the two eclipses. The smallness of the change of magnitude at *e* indicates that the body which is there eclipsed is much darker than its companion, for the loss of its light makes very little difference to the total magnitude of the system.[2] At *b*, however, the darker body passes in front of the brighter one, and there is a much greater fall in brightness. The eclipse begins at *a*, reaches its greatest phase at *b*, and ends at *c*, so that the curve shows the durations of the eclipses as well as the light changes. The sharpness of the minimum points, *b* and *e*, indicates that the eclipses are only partial. If they were total or annular, there would be horizontal lines at *b* and *e*, representing the finite times occupied by the greatest phases. This actually occurs in the light curves of some eclipsing variables; for example, *U* Cephei.[3] The lines *cd* and *fg* are not horizontal, showing that the apparent magnitude changes during the intervals between the eclipses. This is a common feature in the light-curves of eclipsing variables, and shows that the components

[1] Figs. 20, 21, 22, 23, and 27 are taken from *An Introduction to the Study of Variable Stars*, by Caroline E. Furness.

[2] The darkness is only relative. Aitken estimates, from the parallax of Algol, that the fainter body is far brighter than the Sun.

[3] Variable stars are usually indicated by capital letters placed before the genitive of the constellation names. In any one constellation, the variables are named generally in the order of discovery of their variability by the letters *R*, *S*, *T* . . . *Z*; *RR*, *RS*, *RT* . . . *RZ*; *SS*, *ST* . . . *SZ*; *TT*, *TU* . . . *TZ*; . . . *ZZ*; *AA*, *AB*, . . . *AZ*; *BB*, *BC* . . . *BZ*; . . . *QQ*, *QR* . . . *QZ*.

of the star are not spherical, but ellipsoidal, so that the area of the surface they expose varies as they revolve. This is not surprising, for their mutual gravitational influence would tend to give them this shape.

Light-curves of other eclipsing variables are similar to this in their general features, but there are some marked differences in detail, which reveal differences in orientation of orbits and in relative brightness, size, and mass of the components. For example, the intervals *cd* and *fg* may

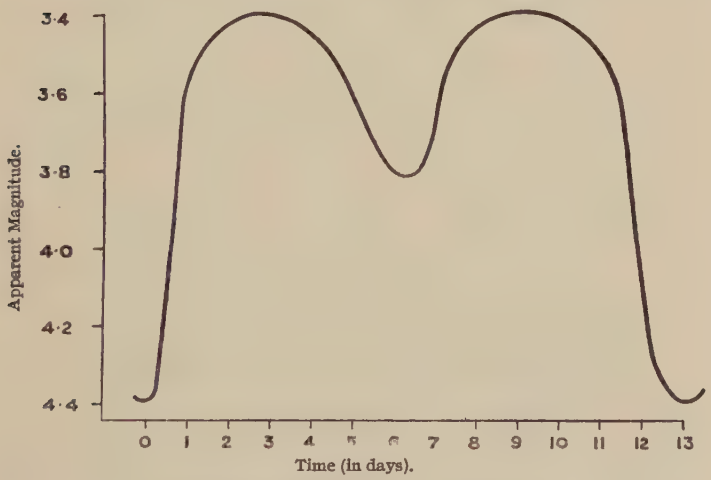


Fig. 21—Light-Curve of β Lyræ

be unequal—indicating an elliptical orbit of a particular kind. One type of curve, in particular, is so unlike Fig. 20, that for a long time it was thought to represent a different kind of variation altogether. It is shown in Fig. 21 for β Lyræ, the most prominent star giving a curve of this kind. It is generally agreed now that β Lyræ variables are eclipsing binaries whose components are ellipsoidal and almost, if not quite, in contact at the ends of their major axes. The star rotates in such a way that the components always present the same faces to one another—like two eggs, touching at their sharpest points, and rotating in a plane through their longest axes.

It will readily be understood that the period of an eclipsing

binary, and the durations of the eclipses, give us some information about the sizes of the component stars relative to the size of their orbits. As we have already explained (p. 129), if we assume a value for the ratio of the masses of the components, the light curve alone is sufficient to reveal the density of the system. In making the calculation, however, allowance has to be made for the possibility that the stars are darker at the limb than they are at other parts of their surfaces —a phenomenon which we found in the Sun and which we must suppose other stars will show as well. In two stars, at least—*Z* Draconis and *RT* Persei—Russell and Shapley consider that "the observations afford conclusive evidence that the stars are really darkened at the limb." We can only speculate as to the amount of the darkening, especially in stars whose spectral types are different from that of the Sun, so that there is an element of uncertainty in the calculated densities on this account.

Eclipsing variables are always spectroscopic binaries, and therefore short-period variables. From one point of view this is unfortunate, because it means that they are mainly of early spectral type, and the special information they yield is therefore confined almost entirely to the early-type stars. It may seem strange that bodies whose variation is due only to the accidental orientation of their orbit-planes, should mostly belong to a particular spectral class. The reason is this: the smaller the distance between the components, the greater will be the chance that their eclipses will be seen from the Earth; in other words, the greater will be the region in space from which the eclipses may be seen. If the stars are actually touching, then an eclipse of some kind will be visible everywhere, except on a line through the point of contact perpendicular to the plane of the orbit. It follows that we shall detect many more eclipsing variables of small separation and short period—and therefore of early spectral type—than of larger separation and later type. The reason lies, not in the intrinsic qualities of the stars themselves, but in the conditions which govern their appearance as eclipsing variables. We may notice, however, that the eclipsing variable having the shortest period known (less than six hours) has recently been found by Jordan to have a mean density between 3 and 5 (water = 1). This suggests a very late type of spectrum.

But there is another point of view from which it is very

satisfactory that eclipsing variables are spectroscopic binaries. This fact enables our interpretation of the light curve to be confirmed, and, moreover, adds to the knowledge we can obtain about the bodies by showing the relative orbital velocity. The evidence of the spectroscope is quite consistent with that of the light curve. At an eclipse, for instance, both components are moving across the line of sight, and the orbital displacement of the spectrum lines should disappear. About midway between the eclipses, however, there should be maximum displacement of the spectrum lines, because the bodies are moving in the line of sight. This is quite in

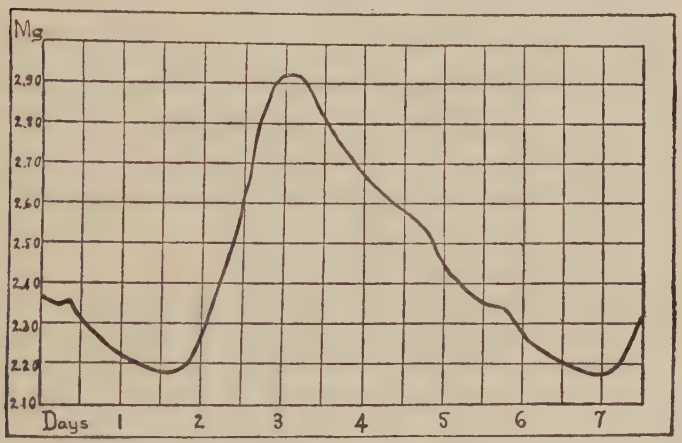


Fig. 22—Light-Curve of δ Cephei

accordance with fact. The agreement is highly satisfactory, because, when the explanation of the variability in terms of eclipses was first advanced, it was not imagined that a confirmation of the idea could ever be obtained. The spectroscope, however, gave a confirmation, and converted a hypothesis into a certainty.

Cepheid Variables.—We should naturally expect that all short-period variables would turn out to be eclipsing binaries, and that the spectroscope would confirm this interpretation. It is something of a shock, therefore, to find a large number of short-period variables in which the spectroscope shows the maximum " orbital " line-displacements when the brightness is at its *minimum* ; *i.e.* when an eclipse should be taking

place. This disposes of the possibility of interpreting the variation as the result of eclipses, and, indeed, the light curves of these variables differ so markedly from those of the eclipsing binaries that such an interpretation seems, on these grounds also, to be out of the question. It is clear that another explanation must be sought.

Variables of this kind were once thought to belong to three classes, but it is now believed that they are all of essentially the same character. The first class—the *Cepheid* variables—takes its name from δ Cephei, a typical member, whose light curve is shown in Fig. 22. It reveals a fairly

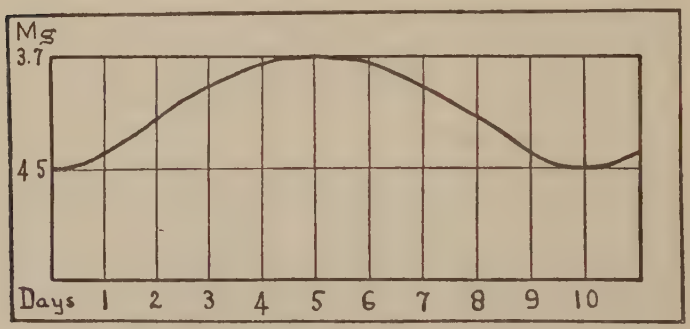


Fig. 23—Light-Curve of ζ Geminorum

steep rise to maximum, and a more gradual and irregular fall to minimum. The second class—the *Geminids*, after ζ Geminorum—shows a similar light curve (Fig. 23), except that the interval from minimum to maximum is equal to or longer than the interval from maximum to minimum. The *cluster* variables—the third class—are mostly found in globular clusters, though a considerable number, *e.g.*, *RR* Lyræ, *RS* Bootis, belong to the general galactic system. Their light curves are identical with those of Cepheids, but their periods, on the average, are shorter. We shall consider all three types together as Cepheids.

The Cepheid variables seem to be a special class of body, with a number of features in common. They may be of any spectral type, but occur mainly among types $G-A$. They all appear to have high luminosity—greater than that of the

average giants of their spectral classes. Their median [1] absolute magnitude appears to be of the order of -2.2. It depends, however, upon the colour-index in such a way that, if one of the stars is brighter by one unit of absolute magnitude than another, its colour-index will show a relative reddening of about 0.35 magnitude. There is also an important dependence of brightness upon period, which we shall discuss presently. The high luminosity of these stars may be very significant in view of the abnormally low luminosity of other giant stars of the same spectral type (see p. 173). The enhanced lines in the spectra of Cepheids are very pronounced, and are narrow and sharp—another indication that the stars are super-giants. The same kind of spectrum is given by a few other stars which are not variables. These stars have been called *pseudo-Cepheids*: they also are extremely brilliant. According to Russell's theory of evolution, the great luminosity of Cepheids would suggest that the stars were exceptionally massive. It has not been possible to measure their masses directly, but it is significant that their radial velocities are lower than the average values for their spectral types. The densities of the stars also appear to be very low. In spite of their great absolute brightness, Cepheid variables are apparently faint stars, so that their distances must be very great: they are evidently not scattered indiscriminately throughout the Universe. They show a marked galactic concentration, as, indeed, do the eclipsing variables also. This would have no special significance in itself, for we know that the stars as a whole are strongly condensed to the galactic plane, but the concentration of these bodies is much greater than the average, so that some special meaning must be attached to it. It may be an effect of great distance, as a glance at Fig. 13 will show.

The periods of Cepheid variables extend from a few hours up to about 50 days. The shortness of period is one of the chief points of distinction between Cepheids and other variables which we shall discuss presently. Together with spectral type—and, to a less extent, the light-curve—it leaves no ambiguity with regard to the classification of a variable. The range of variation of brightness may be anything from $\frac{1}{2}$ magnitude up to about $1\frac{1}{2}$ magnitudes. There is no obvious connection between this range and the period of variation.

[1] The *median* magnitude is the average of the magnitudes at maximum and minimum brightness.

A very remarkable relation, however, exists between the period and the median absolute magnitude of Cepheids. The first indication of the relation appeared in a study, by Miss Leavitt, of the Cepheid variables in the lesser Magellanic Cloud—a remarkable region of space which seems to contain samples of every kind of cosmic body (see p. 351). Miss Leavitt found that the logarithm of the period of variation bore a simple relation to the median apparent photographic magnitude. If these two quantities were plotted on a diagram, for all the Cepheids then known in the Cloud, they lay very nearly on a smooth curve (see Fig. 24). The lesser Magellanic Cloud is very far away compared with its dimensions, so that all the variables considered were practically at the same distance from the Earth. Their apparent magnitudes were therefore a measure of their absolute magnitudes. In the absence of accurate knowledge of the distance of the Cloud, the absolute magnitudes could not be calculated exactly, but it nevertheless followed that the relationship found by Miss Leavitt was a relationship between absolute magnitude and period, though the zero point on the magnitude axis could not be fixed.

It was afterwards announced by Shapley that the relation was only a particular example of a much more general one, which applied to Cepheid variables in any part of space in which they could be detected. Selecting eleven typical Cepheids from our own galactic system, Shapley found their mean parallax from the υ-component of the proper motions, in the manner described on p. 110. Then, from the measured apparent visual magnitudes, the absolute visual magnitude of each star was calculated. The validity of this process, of course, depended on the assumption that the individual parallaxes were not greatly different from the mean ; *i.e.* that there was very little " dispersion " in parallax among the stars. Shapley took steps to eliminate the effect of such dispersion as far as possible, but his justification for determining *individual* parallaxes from proper motions has, nevertheless, been strongly criticised, particularly by Curtis.

From the results he obtained, however, Shapley found that the absolute magnitudes of the eleven galactic Cepheids were connected with the periods in a manner similar to that found by Miss Leavitt for the Cepheids in the lesser Magellanic Cloud, provided that Miss Leavitt's magnitudes were changed from the photographic to the visual scale by means of the

colour indices ; and, in addition, he knew the actual absolute magnitudes of the galactic stars, and could therefore fix the zero point on the magnitude axis of the diagram. Assuming, then, that Miss Leavitt's curve and his own were not only parallel but actually coincident over their common

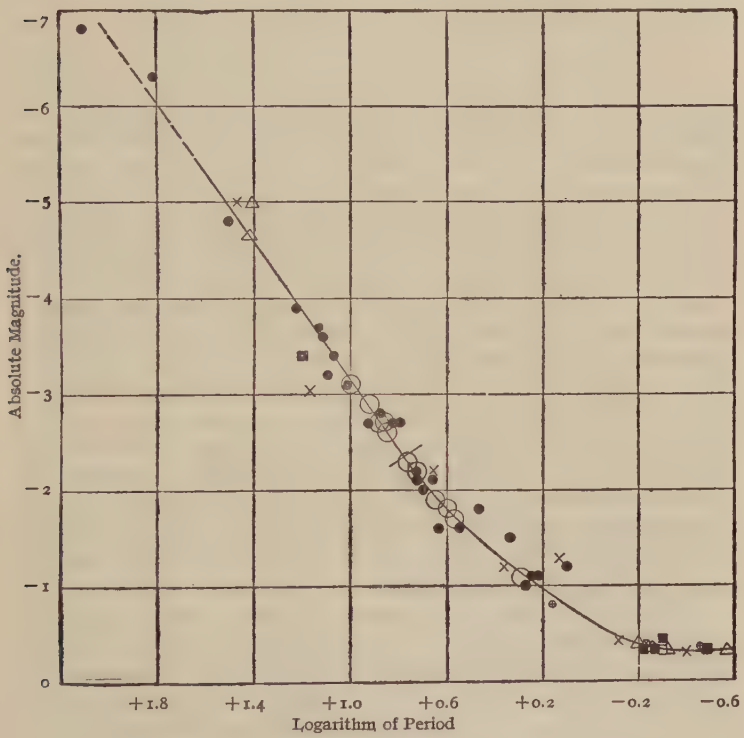


Fig. 24.

Luminosity-Period Curve for Cepheid Variables.

range, he could fix the absolute magnitudes of the Cepheids in the Cloud, and draw a single curve by combining the two sets of data. He next turned his attention to a number of globular clusters, in which Bailey had previously shown that variable stars similar to Cepheids existed in considerable numbers, and he found a precisely similar relation there. In each cluster, as in the Magellanic Cloud, the stars could be considered to have the same parallax, so that the apparent

magnitudes were again a measure of the absolute magnitudes. The *actual* absolute magnitudes were, as before, assumed to be such that the curve for each cluster was coincident with the curve for the galactic Cepheids. Combining, then, all the data into a single curve, Shapley obtained the diagram shown in Fig. 24, in which the various marks (circles, dots, etc.) represent the data from different groups of Cepheids.

Taking a general view of the process, the position appears to be this. From actual observation, without any assumption whatever—except that the stars in any one globular cluster or in the lesser Magellanic Cloud can be considered to have identical parallaxes—there is a definite relation between the period and absolute magnitude of Cepheid variables, wherever they are found outside our general galactic system—a relation which, though it may be that there is a different absolute luminosity for a given period according to the cluster or Cloud in which the Cepheid is situated, shows, nevertheless, that the *change* of luminosity for a given *change* of period is the same for the Cloud and all clusters. If we can trust individual parallaxes determined from the not very widely dispersed proper motions of eleven stars, then a similar relation holds for *galactic* Cepheids. Assuming, finally, that the similar relations for the various clusters, the Cloud, and the galactic system are absolutely identical, so that the absolute magnitude of a Cepheid of given period is the same wherever the star may be found, we arrive at the luminosity-period curve shown in Fig. 24.

Two distinct assumptions are involved, either of which, standing apart from other relevant facts, might be considered to be somewhat rash. The actual circumstances, however, seem to give them a high degree of justification. In view of the undoubted relations between period and luminosity found in Cepheids everywhere but in our own system, it would be very surprising if our own Cepheids, with otherwise similar characteristics, should exhibit no such relation. The fact that the parallaxes determined from proper motions lead to absolute magnitudes obeying precisely the expected period-luminosity relation therefore makes it highly probable that the parallaxes so obtained are at least relatively accurate. They might, of course, all be subject to the same systematic error, if the eleven galactic Cepheids, as a body, have a proper motion with respect to the rest of the galactic system, but this Shapley has shown to be very

improbable. If it be granted, then, that the galactic Cepheids resemble the others in the exhibition of a similar luminosity-period relation, it is difficult, in the absence of any argument to the contrary, to avoid the conclusion that these similar relations must be identical. The point is one of extreme importance, because, as we shall see, the farthest-reaching consequences follow from it, and their validity depends in very great measure on the reality of Fig. 24.

At the present moment opinion is divided on the matter. The division seems to depend on the relative importance which is attached to different types of argument. If, regardless of consequences, one examines ruthlessly the *a priori* justification for adapting statistical processes to individual cases, he will necessarily conclude that Shapley's parallaxes are entitled to little weight, and therefore that the whole of the subsequent argument for the reality of Fig. 24 must fall to the ground. If, on the other hand, one takes the uniformity of Nature into account, and, from the behaviour of cluster Cepheids, thinks it right to expect a similar behaviour in galactic Cepheids, then, whatever the abstract legality of Shapley's procedure in general might be, he will conclude that the results in this particular case justify its employment and establish a sound basis for further advance. Until some means is found by which the absolute magnitudes of galactic Cepheids can be determined beyond question, the difference of opinion will probably remain.

But there is another kind of criticism of Shapley's work, to which Kapteyn and van Rhijn have called attention, which is entitled to very serious consideration. It depends on a possible difference in type between the very short period Cepheids, which greatly predominate in globular clusters, and the Cepheids of somewhat longer period which form the majority of those in our own system and in the smaller Magellanic Cloud. The members of the former class which occur in the galactic system show no galactic concentration, have fairly large proper motions, and are therefore probably not very distant. They are apparently very faint, however, so that there is a strong argument for assigning to them a low absolute luminosity. From the available parallactic motions of fourteen of them, Kapteyn and van Rhijn deduce a mean parallax 7.6 times as great as that found by Shapley for the longer period Cepheids—a figure agreeing closely with an independent determination by Schouten. This, of

course, indicates that the absolute brightness of these stars is much less than Shapley assumed it to be.

If this criticism is a valid one, its effect must be to establish two different curves connecting period and absolute luminosity —one for the longer period Cepheids of our own system and the lesser Magellanic Cloud, and the other for the shorter period Cepheids which abound in clusters and are found in much smaller proportions in the galactic system. These two curves will not run smoothly one into the other: the effect will be to move the horizontal part of the curve in Fig. 24 much lower down in the diagram, without changing the other part of the curve. It is true that, in certain clusters, Shapley has found longer period Cepheids, which fall on a curve continuous with that for the short period Cepheids in the same clusters. This is evidence that there is really only one curve: Kapteyn and van Rhijn, however, consider it to be based on too small a number of stars to be accepted, in view of their determination of the mean parallax of the shorter period galactic Cepheids.

Shapley is not prepared to accept this criticism. He gives reasons for assigning high space velocities to the short period galactic Cepheids, and he suggests that the large proper motions of the stars are probably due to this cause rather than to nearness. He points out that parallactic motions are unsuitable data for the determination of parallaxes of stars with high velocity (see footnote on p. 111), and maintains that the probability of a "parallactic drift" resulting from the systematic motion of stars of high velocity (p. 97) would seriously affect the results of Kapteyn and van Rhijn. It is unfortunate that the available evidence is not sufficient to settle the question definitely.[1]

Whatever the truth in this matter might be, however, the period-luminosity relation in itself gives a peculiar interest to the problem of the origin of Cepheid variation. This has for long been one of the great puzzles of Astrophysics. It

[1] R. E. Wilson, after a careful study of the proper motions of all the Cepheids in our own Galaxy for which data are available, finds that the stars considered by Kapteyn and Van Rhijn have considerably larger space velocities than those for which parallaxes were determined by Shapley, and that Shapley's parallaxes are, at the most, only 40% too small. Shapley points out that if the Kapteyn corrections be applied to the Boss proper motions used by Wilson, the results of Wilson's investigation indicate that the parallaxes should not be increased at all, but, if anything, slightly decreased.

is not, as we have seen, that the phenomenon lacks characteristics: on the contrary, it has a large number, which distinguish the Cepheid variables quite sharply from other stars. The trouble is that the characteristics are exceedingly difficult to reconcile with one another. The light-curve, it is true, is periodic, but the variation throughout a period suggests nothing with regard to its cause. The spectroscope indicates that the stars are binaries: the periodic displacement of the spectrum lines is identical in kind with that observed in an ordinary spectroscopic binary, though no trace of a second spectrum has ever been seen. If we suppose the stars to be binaries, and compare the light-curve with the spectrum, it appears that a Cepheid is brightest when the orbital motion of the bright component is directly towards us, and faintest when the orbital motion is directly away from us. This is very puzzling, and, as we have pointed out, seems to rule out the possibility of explaining the variation in terms of eclipses, the natural interpretation of combined displacement and variability. Can we, then, dissociate the two characteristics—accept the stars as binaries and look for a quite independent cause of variation? This again is impossible, for the periods of oscillation of the spectrum lines are identical with the periods of the light-curves. The two phenomena are undoubtedly connected, and the true explanation must account for them both.

It is impossible here to deal with all the explanations that have been proposed: we must perforce omit some and content ourselves with a bare mention of others, reserving the greatest space for the latest hypothesis. It has been suggested that a Cepheid variable is a binary star of which the bright component is the less massive. Its companion, round which it revolves at a fairly small distance, must raise considerable tides in it, and the resulting changes in the area of surface exposed have been held to account for the variation of brightness. Another idea is that the orbital motion of the two component stars takes place in a resisting medium which, through heat developed by friction, makes the front of the luminous star brighter than the rear, and so causes maximum brightness during approach. An obvious objection to this is that the companion star should be heated in the same way, and, moreover, a resistance capable of accounting for the light variation would have the effect of changing the period of the star to an extent which is not observed. A third

attempt at explanation appeals to solar phenomena for help. It suggests that the orbit of the binary star is filled with a very rarefied material, like the Sun's corona. The front of the bright star is conceived to appear brighter than its rear, not because of heat developed by friction, but because the absorbing atmosphere of the star is brushed away to some extent by the coronal medium. The photosphere is then exposed, and is able to give more nearly its full luminosity.

In all these explanations it is assumed that the oscillating spectrum lines indicate that the stars are binary. In 1914, however, Shapley challenged this assumption. He pointed out several objections to it, and argued, very reasonably, that it had never led to a satisfactory explanation of the variation. Among the objections, he included the very low value of the "mass-function," to which we have referred before (p. 239). The *universal* absence of a secondary spectrum also was very curious if the stars were really binaries. Further, if the stars were binaries, it would have to be granted that the orbits were very small: their radii would average less than one-tenth of the radii of the stars themselves, so that the stars would have to overlap during the revolution. Then, again, the period of a binary star should be strictly constant, or, if a resisting medium were assumed, it should vary always in the same direction. As a matter of fact, however, small variations of period were quite general among cluster variables, but the *mean* periods, taken over a considerable range of time, were constant in almost all such stars. Moreover, the irregularly changing form of the light-curve seemed to demand something other than a binary star explanation. Finally, Shapley pointed out that the periodic variation affected, not only the apparent magnitude and the radial velocity, but the spectral type as well. He held that this pointed to a cause more intimately connected with the stars themselves than the mere position in an orbit could be.

The last point was evidently of very great importance, and demanded close investigation. Mrs. Shapley showed that there were five qualities in which Cepheids underwent periodic variation of essentially the same kind; namely, photographic apparent magnitude, photovisual apparent magnitude, colour index, spectral type, and radial velocity. Fig. 25 shows graphically the extent of the variation. The similarity of the five curves is almost a convincing argument that we are dealing here with a phenomenon which arises from the

essential character of a star, and not with anything that depends to any great extent on the position in space from which it is seen. It was found a little later by Adams and

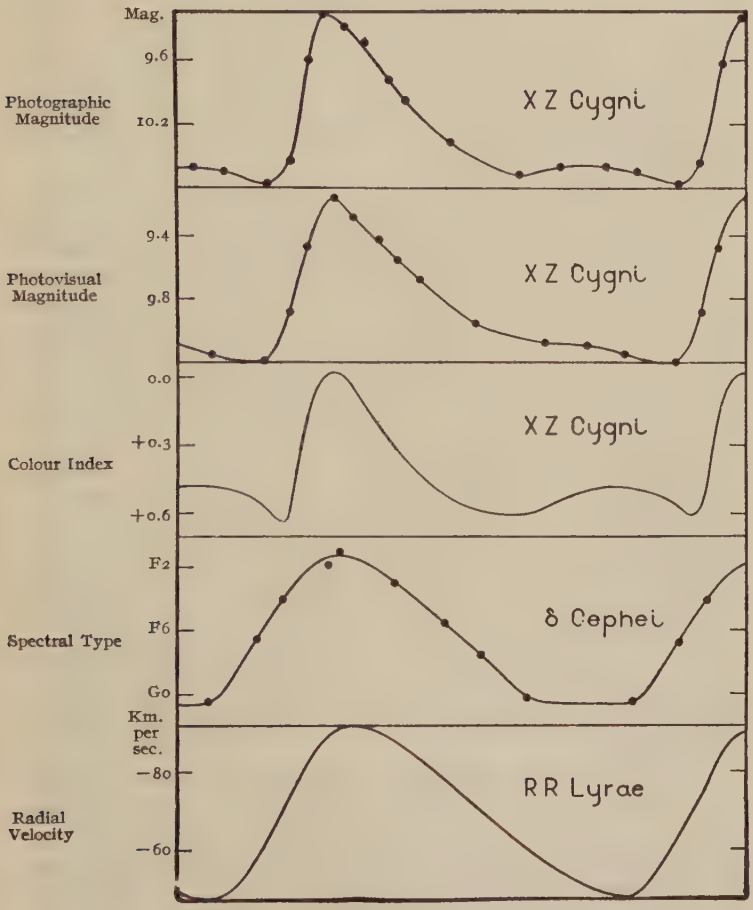


Fig. 25.

Five Aspects of Cepheid Variation.

Joy, and corroborated by Seares and Shapley, that the change of spectral type was confined to a change in the intensity of the hydrogen lines, the other part of the spectrum remaining

practically constant. Nevertheless, Albrecht still maintains that there is a general change of spectral type—in the star, *l* Carinæ, from *F*8 near light minimum to *G*9 near light maximum—and he associates the change with the wave-length alterations which, he claims, occur generally with change of spectral type (see p. 101). However this may be, there is no doubt that a spectrum change of some kind accompanies the light change of a Cepheid, and this, in itself, casts grave doubt on the adequacy of any binary star explanation of the variation.

A thoroughly satisfactory alternative hypothesis, however, is not easy to find. Shapley proposed that the cause of the phenomenon might be a periodic pulsation in a single star, without specifying the origin or fundamental character of the pulsation. He suggested at first that the variations could be explained by supposing that " a surface of approximately constant area progressively changes its spectral type as the result of a periodic flow and ebb of heat." Later, he defined the pulsation as an alternating radial expansion and contraction of the star, in order to account for the Doppler displacement of the spectrum lines. Such a process, however, would cause, not a simple displacement of the lines, but a broadening, first on one side and then on the other; for during expansion or contraction, the components of velocity in the line of sight would vary continuously from zero at the limbs to a maximum in the centre of the disc. Shapley and Nicholson have examined this point, and they find that, in the necessarily imperfect state of recorded stellar spectra, the appearance of the lines would probably be indistinguishable from that of lines which are simply displaced without broadening.

Shapley has shown that, if a Cepheid variable is a single star pulsating in a period which is related in a certain simple manner to its mean density, then the general features of the period-luminosity relation for Cepheids of longer period can be explained. The most complete theoretical discussion of the pulsation hypothesis, however, has been made by Eddington. Assuming that the pulsation takes the form of a symmetrically alternating dilatation and contraction of the star, he finds it possible, along the lines of his general theory of stellar interiors, to deduce the actual value of the period for a given magnitude and spectral type; the asymmetrical character of the velocity curve derived from the

displacements of the spectrum lines; and also the fact that, the more luminous the star, the redder it is. His results for all these phenomena agree closely with observation. He has shown also that the maximum brightness must occur after the time of greatest compression, and, therefore, during the expansion of the star, but he has not yet been able to prove that it must take place when the velocity of expansion is greatest, as observation indicates. The extent of the expansion or contraction appears to be from 5 to 11 per cent. of the star's radius, and the light change is roughly proportional to this. The period of pulsation is found to depend on the mean density of the star, and from this result Eddington makes an interesting addition to the evidence that the rate of stellar evolution is exceedingly slow. If, as we believe, the stars are increasing in density with advancing age, then the periods of Cepheids, according to the pulsation hypothesis, should be decreasing. There is no evidence, however, that, in general, the periods tend continuously to change at all—a fact which is interpreted as an indication that the changes are too slow to have become noticeable within the duration covered by our records. It has been suggested that Cepheid variation is the symptom of a condition through which all stars must pass at a particular stage of their lives. Eddington finds, however, that, if the pulsation hypothesis be true, this notion is untenable: certain special physical conditions, probably in the outer layers of a star, must exist before the variation can occur.

On the whole, the pulsation hypothesis holds the field at present. It offers a more plausible explanation of the phenomena, and is less open to serious objection, than any of the binary star explanations. It awaits now the more complete understanding of the conditions in the interior of a normal star which we are steadily acquiring.

Long-Period Variables.—Turning from Cepheids to another large class of variable stars—the *long-period variables*—we observe, first of all, that there is a conspicuous gap in length of period between these stars and the variables we have already considered. The gap serves to distinguish, in name, the long-period variables from the Cepheids and eclipsing binaries, but the physical distinction does not depend entirely on period: spectral type has as much, or more, weight in deciding in which class a star is to be placed, and there are other

characteristics in which the classes are sharply divided. Eclipsing variables, as we know, are mainly of early type, while the greater number of Cepheids belong to types $G-A$. Long-period variables, however, with the exception of a few stars which are classified in a sub-group, are almost completely confined to classes M and N.[1] Bright hydrogen lines invariably appear in the spectra—sometimes accompanied by bright lines of other substances. SZ Cassiopeiæ, with a period of fifty days, might so far as period is concerned be regarded as a Cepheid. Its spectral type, however, proclaims it a long-period variable with a period much shorter than the average.

The galactic concentration of long-period variables is less than that of the stars as a whole. It is not clear that any special significance lies in this fact: it shows, however, that there is a marked contrast in the distribution of Cepheids and long-period variables. Another distinction between these bodies is that there is no oscillation of the spectrum lines of long-period variables, and therefore no possibility of a binary star interpretation of the light changes. This fact possibly smooths the path of the pulsation theory of Cepheids, for it shows that periodic variation of a single star is possible. The light-curve of a long-period variable shows, as a rule (see Fig. 28), an accelerated rise to maximum, followed by a slow and fairly regular descent. It is not unlike the curve showing the variation of sunspot areas (Fig. 17), but the resemblance between the phenomena seems not to extend any farther.

The star Mira (o Ceti) is the best-known variable of this class. The great range of variation—about seven magnitudes—makes the changes conspicuous to a close observer of the sky, and led to their discovery so long ago as the sixteenth century. The period is about 332 days, but it is subject to considerable fluctuation—a general characteristic of long-period variables. These stars, in fact, depart in several respects from the close regularity exhibited by the short-period variables. Though the average characteristics are fairly constant, there are marked irregularities, not only in the length of period, but also in the range of variation and in the actual brightness at maximum and minimum. It is probable that the spectral type changes during the transition from maximum to minimum brightness: it is known to do

[1] The new class S contains a number.

so in the case of Mira, whose spectrum at minimum is unlike any other known spectrum. The continuous background in the spectrum of this star also appears to be brighter at maximum than it is at minimum, and all the long-period variables become redder with diminishing brightness.[1]

On the whole, the characteristics of long-period variables seem to be less numerous and definite, but more immediately

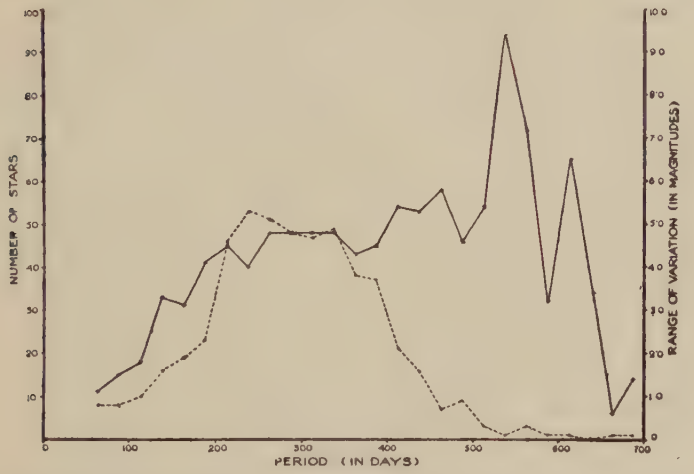


Fig. 26.

Graphs showing number (dotted graph—scale on left) and range of magnitude variation (continuous graph—scale on right) of long-period variables having different periods.

obvious, than those of Cepheids. There is, however, a marked correlation between length of period and range of variation in the former class which is conspicuously absent from the latter. It appears that, with some irregularities, there is a general tendency for the range of variation to increase with length of period. This is shown clearly by the continuous graph in Fig. 26. The dotted graph on the same diagram shows the number of stars of each period, revealing a preference for periods of 200-350 days. The " range of variation " adopted for each period is the average for all the stars indicated by the dotted graph as having that period. The

[1] Mira has recently been found to be a visual binary star, but it is not yet clear exactly to what extent this fact may account for the remarkable phenomena by which it is characterised.

colours of long-period variables show interesting correlations with length of period and apparent magnitude at maximum. In the first place, the longer the period, the redder is the star: in the second place, the smaller the apparent brightness at maximum, the redder is the star. Long-period variables generally have a decidedly reddish colour. These relations are evidently too well marked to be accidental, and they should be necessary consequences of any theory which aims at a comprehensive explanation of the variability.

The classification of long-period variables is a matter of some difficulty. There are two types which differ conspicuously from the majority, and these have been classed in sub-groups known respectively as II.*b* and II.*c*[1] Class II.*b* includes the stars *U* Geminorum, *SS* Cygni, *SS* Aurigæ. They are all faint stars, with peculiar light-curves. The normal state is one of minimum brightness which is constant for a considerable length of time. Suddenly, however, at intervals which cannot be predicted, the brightness rises rapidly to a maximum, and immediately begins to fall again, much more slowly, to the steady minimum. In *U* Geminorum and *SS* Cygni, the period of time occupied by the combined rise and descent is not the same at all variations. There are two distinct periods, which occur alternately in *U* Geminorum and generally in *SS* Cygni also (see Fig. 27). The latter star, however, has a third type of variation, represented by a fairly symmetrical curve: its occurrence follows no known law. *U* Geminorum and *SS* Cygni have peculiar spectra, somewhat similar to the *F* type. *SS* Aurigæ may resemble them in this respect, but it is too faint for the spectrum to be observed clearly.

Class II.*c*, also consisting of three stars—*R* Coronæ Borealis, *RY* Sagittarii, *SU* Tauri—shows equally peculiar characteristics, but in the normal state the stars have maximum instead of minimum brightness. The luminosity remains constant, sometimes for a year or more, and then, without warning, falls to a minimum, subsequently recovering its full measure slowly and irregularly. Both the duration

[1] After Pickering's classification of variable stars, which was as follows: Class I.—Novæ. Class II. (with sub-groups II*b*. and II*c*.)—Long-period variables. Class III.—Irregular variables. Class IV.—Short-period variables, including Cepheids and variables of β Lyræ type. Class V.—Eclipsing binaries. This classification is now in need of a slight amendment, since it is generally believed that β Lyræ variables are eclipsing binaries.

of faintness and the range of variation of magnitude are subject to large irregularities.

The great bulk of long-period variables reveal no qualities obviously suitable as a basis for classification. Nevertheless, the Rev. T. E. R. Phillips has shown that it is possible to divide them into two distinct groups. He has done this by subjecting the light-curves to mathematical analysis; the details of his classification are therefore beyond our scope. The division is partly reflected, however, in the general appearance of the light-curves, so that we may see the results, if not the process, of Phillips's analysis. In the light-curves of group I. the interval from maximum to minimum is almost equal to

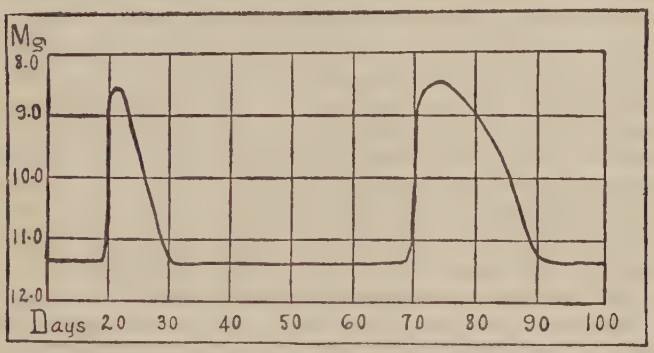


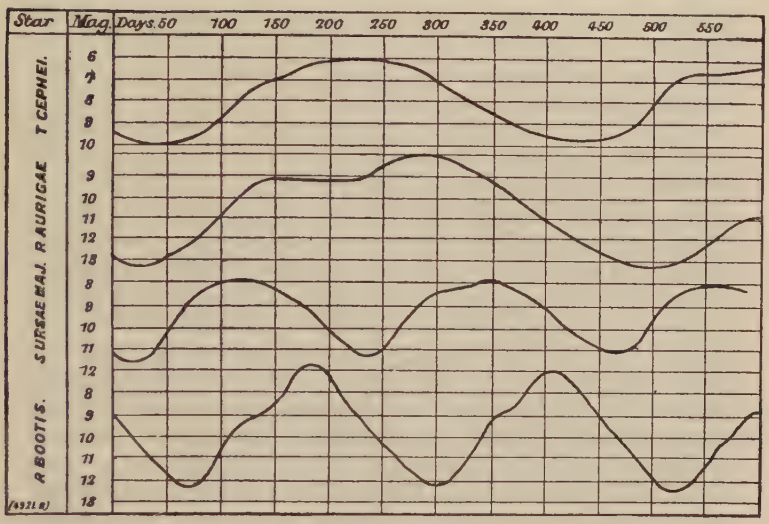
Fig. 27.—Light-Curve of *SS* Cygni.

the interval from minimum to maximum. The curves, however, are not, as a rule, symmetrical, because there is usually a pause (represented by a short horizontal line), on the upward branch. The group II. curves show a steep rise to maximum, followed by a much slower descent to minimum. Fig. 28 contains light-curves representative of the two groups. It must be understood, however, that there are considerable differences among the curves of each group. The stars of group I. have, on the average, a smaller range of variation than those of group II., and have a mean apparent brightness nearly 2.3 times as great.[1]

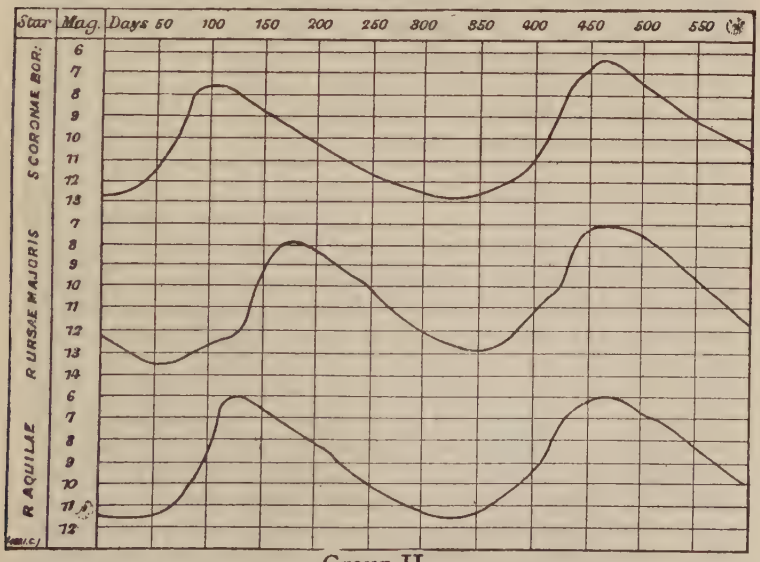
[1] Father Hagen, of the Vatican Observatory, has subdivided Phillips's group II. into two classes—(*a*) with light-curves showing broad minimum; (*b*) with light-curves showing a very rapid rise to maximum.

Turner has followed up Phillips's classification by a study of the periods of the stars in relation to the group division. He has made a special study of the star *W* Cygni, which he has shown to possess two concurrent periods, of 130 and 243 days, respectively, the former of which has the characteristics of group I., and the latter those of group II. This has led him to the idea that long-period variation undergoes a development along the following lines. Starting in group II., with a period of about 400 days, a star progressively acquires a shorter period. The process continues until the period is about 200 days, when the star moves into group I., and its period, starting at about 150 days, begins to lengthen again. It should be observed that the two groups are not conspicuously distinguished by length of period: the same periods occur in both. But when a star has a short period, it is, according to this idea, at the junction of the definitive characteristics of the two groups. *W* Cygni is now at the junction. The two periods are the dying one of group II. and the newly-born period of group I. which will gradually lengthen as the star grows older. Turner finds, in agreement with his hypothesis, that, generally speaking, the periods of the group II. stars are decreasing while those of the group I. stars are increasing. In both groups, the stars of longer period have the higher galactic concentration.

The cause of variation in long-period variables is perhaps still less understood than is the cause of Cepheid variation. An explanation has been proposed for the variation of Mira which arises from an observation of Campbell's, that at maximum brightness, the bright hydrogen lines in the spectrum show a velocity of approach nearly $9\frac{1}{2}$ miles per second greater than the velocity shown by the other lines. It is suggested that the variation is due to periodic outbursts of hydrogen—like huge prominences—which reach beyond the atmosphere of the star and so give bright spectrum lines. There appears to be no evidence of the same kind for the other long-period variables. It should be mentioned, however, that the great majority of these stars show bright lines in their spectra—including always one or more hydrogen lines, and sometimes lines of other substances. Merrill has pointed out that, in every long-period variable of type *Md* whose spectrum has been adequately observed, the following bright lines appear, in the order given: $H\delta$, $H\gamma$ (hydrogen), $\lambda\lambda$4202, 4308 (iron), λ4571 (magnesium). The behaviour of the lines



Group I.



Group II.

Fig. 28.

Light-Curves illustrating Phillips's Classification of Long-Period Variables.

appears to be substantially the same in the spectra of all the stars in which the lines appear, including that of Mira: it seems, in fact, to be characteristic of all *Md* stars. Adams and Joy have stated that the bright lines are all arc or flame lines, but the identification of some of them is perhaps doubtful. These observations seem to complicate the application of the prominence explanation to long-period variables in general: more evidence seems to be called for with regard to the radial velocities indicated by the respective lines in a number of stars. But even if the explanation should prove adequate, the cause of the outburst of gases would still remain a mystery.

Merrill has put forward a hypothesis which takes into account the act that these stars are confined to the latest spectral types, and has the further merit of giving a natural explanation of the correlation between length of period and range of light variation. He supposes that the outer portions of the stellar atmospheres condense into clouds—a process which might be possible in the cool stars of late type, but not in hotter stars like the Sun. If the clouds consisted of small particles, they would scatter as well as obscure the stellar light, and would treat the shortest wave-lengths most severely, so that the stars would appear abnormally red. The confined radiation would heat the clouds and at length vaporise them and escape. The times at which this would occur would be the times of maximum brightness. The clouds would then form again, and the process would be repeated. It is not difficult to imagine that these changes would take place in periods of approximately equal length; and, further, the longer the period, the denser would be the cloudy veil, and therefore the greater the range of light variation. This is a very attractive hypothesis, but its premises seem inherently unlikely. The majority, at least, of long-period variables are giant stars, and the very low density of their atmospheres would not favour condensation. Moreover, there is some difficulty in applying the hypothesis to certain details connected with the bright lines appearing in the spectra.

Turner has investigated the possibility of explaining long-period variation in terms of faculæ—bright clouds like those seen around sunspots when they are near the edge of the Sun's disc. He assumes that there are faculæ on the surface of a long-period variable, which move slowly and periodically from higher to lower latitudes, and then break out again suddenly in the higher latitudes—just as we observe the spots

to do on the face of the Sun (see Fig. 17). A movement of this kind would cause the apparent brightness of a star to vary according as the faculæ were at the centre or limb of the stellar disc or at the back of the star. Obviously, the appearance would depend on the direction of the axis of the star with respect to the line of sight, and Turner finds that the hypothesis fits the observations best if it is supposed that there is a general tendency for the axes to lie in the plane of the Galaxy. There seems to be no fatal objection to this idea, but it cannot be said that, as yet, any explanation of long-period variation has met with general acceptance.

Irregular Variables.—The same remark can be made of the next class of variables—the irregular ones, so-called because no periodicity has been detected in their variation. Some of them, it is true, might be classed as irregular because they have not been observed sufficiently for their periodicity to be detected. This has certainly occurred in the past; for example, *u* Herculis, originally thought to be irregular, was afterwards found to be a spectroscopic binary of β Lyræ type, with a period of 2.05 days. But there can be no doubt that the majority of irregular variables do belong to a class of their own, and though, as observations accumulate, some kind of regularity might reveal itself in their changes, it will be of a much more complicated kind than any we have so far considered.

Irregular variables show more resemblance to long-period variables than to any other class. They are generally red stars—almost all of them have spectral type *M* or *N*. None of them shows a decidedly variable radial velocity, so that a binary star hypothesis, even if it were capable of explaining the variation, would not be available. They are mostly giant stars—Betelgeuse (α Orionis), proved to be a giant in dimensions by the interferometer, is perhaps the best known irregular variable. The range of variation is usually small, in sharp contrast to that of the long-period variables.

It has been proposed that, since the stars are red, and therefore cool, they might be in the initial stages of condensation into solid bodies. Condensation would occur first at the surface, and would be attended by a fall in brightness. But eruptions from the hotter interior would frequently burst through the embryonic crust, causing the star to appear brighter. Such eruptions would not necessarily occur with

any regularity, so that the absence of a periodic variation offers no serious difficulty to this hypothesis. The idea, however, which bears an obvious similarity to Merrill's interpretation of long-period variation, appears to have no independent witness to its truth, and is open to one very strong objection. Giant stars, which form the majority of the class, are believed to be getting hotter, and therefore are not likely to be in the course of condensation. It is true that if they are in the last stages of vaporisation from an originally solid state, the essential feature of the hypothesis remains unharmed. But this is extremely unlikely—one might almost say impossible—because the densities of giant *M* stars are so exceedingly low that spasmodic condensation at the surface is inconceivable. The cause of the variation, at least so far as the giants are concerned, must be sought in some other direction.

Unfortunately, no other direction seems open, unless it be the one to which recent observations of Betelgeuse point. Since the interferometer has shown itself capable of measuring stellar diameters, it has been turned on several occasions to Betelgeuse, and if its statements are unequivocal we must conclude that the diameter of Betelgeuse is varying. If this is so, then the surface of the star must be varying in size, and consequently the total luminosity of the star will probably be varying also. Such an explanation would resemble the pulsation theory of Cepheids, except that the changes here must be supposed to be irregular, and show no clear evidence of variable radial velocity. The latter point seems to be against the general application of this idea, though, it is true, the small ranges of variation would imply relatively small surface changes, and possibly too slow an expansion or contraction to produce an obvious displacement of the spectrum lines.

One of the most interesting of the irregular variables is the star η Carinæ,[1] whose spectrum is very peculiar, resembling that of a nova. The history of this star forms one of the most remarkable passages in the annals of Astronomy. η Carinæ has been on several occasions among the brightest objects in the heavens. At present it is too faint to be seen with the naked eye. The changes have been enormous, but as a rule they have occurred rather slowly—unlike the lightning glories of a nova. The star is situated in a nebula

[1] Formerly known as η Argus.

into which Campbell thinks it likely that it has rushed, with the result that its atmosphere has expanded and become intensely heated. To this atmosphere he ascribes the bright line spectrum. η Carinæ may be regarded as a link between irregular variables and novæ, though, standing alone as it does, it emphasises the division rather than the connection between these bodies.

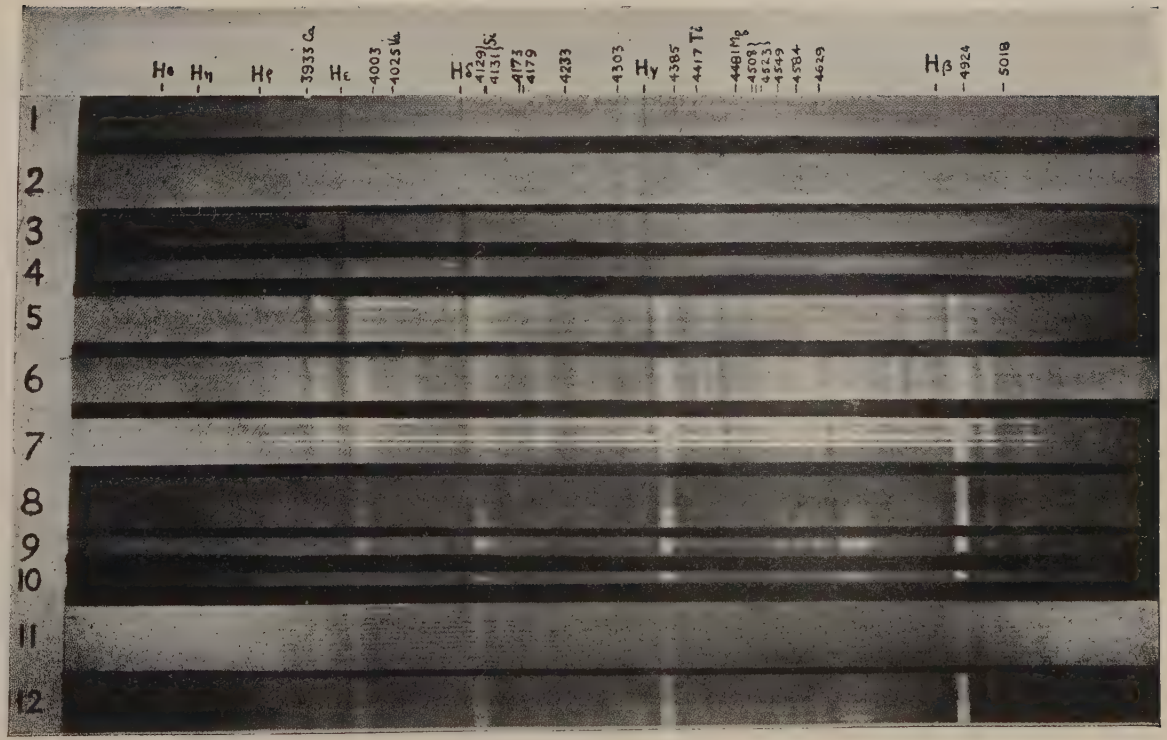
Novæ.—Novæ were classed by Pickering as variable stars. It is doubtful, however, if such a classification expresses anything more than the philosophical instinct which craves after generalisation. So far as its symptoms are concerned, a nova may be called an irregular variable of a very special kind: in origin and nature, the modern tendency is to regard it as quite a unique kind of object, in that it owes its characteristics to external influences rather than to its own peculiar constitution. Very few novæ were recorded stars before their outburst, and the most important of these—*T* Coronæ—as we shall see, differs in several respects from the typical nova. Recent research seems to indicate that novæ are associated more with Wolf-Rayet stars and planetary nebulæ than with variables. We shall, therefore, follow up our account of them with a discussion of Wolf-Rayet stars as a class, leaving the planetary nebulæ for the next chapter.

Novæ, or temporary stars, are bodies which rise from obscurity to great brilliance in a few days, and then slowly and irregularly die down into an apparently permanent state of faintness (but not total darkness) again. Sometimes they are near enough to appear bright to the naked eye—it was one of these bodies, which is reported to have rivalled Venus in brilliancy, that re-directed the attention of Tycho Brahe to Astronomy. Novæ occur almost exclusively in the direction of the Milky Way.[1] They appear spasmodically, without warning, so that, unfortunately, they have usually undergone considerable development before being detected. They are not by any means such rare bodies as is generally supposed. Since photography has been applied systematically to the heavens, they have been discovered at the rate of nearly one per year. Comparatively few, however, become conspicuous to the naked eye.

[1] We are dealing, in this chapter, only with the novæ in our own galactic system: those occurring in spiral nebulæ or the Magellanic Clouds will be dealt with later.

The spectrum of a nova undergoes a series of rapid and extraordinary changes which, in its main outline, is common to almost all novæ. On the few occasions on which such a star has been discovered near the beginning of its career (say, a couple of days before maximum brightness), it has been found to show at first a continuous spectrum, on which it is just possible to trace faint absorption lines, chiefly of hydrogen and ionised calcium, much displaced to the violet. If any bright bands are present, they are hardly discernible. The absorption lines soon become stronger, new ones appear, and the spectrum resembles very closely the peculiar *A2* type represented in the star α Cygni. The lines are due chiefly to hydrogen and ionised titanium and iron: they are greatly displaced to the violet, and are bordered on the red side by very broad and faint emission lines, whose centres are almost coincident with the normal positions of the displaced absorption lines. This is the appearance at about the time of maximum brightness. From this time onwards, the continuous background grows fainter. The absorption lines therefore tend to be less pronounced, and show a gradual drift towards their normal positions. The broad bright lines at the same time become more prominent. Two or three days after maximum, a new absorption spectrum develops—of type *B2*, like the spectrum of Bellatrix (γ Orionis)—whose lines are displaced even more towards the violet than were the original ones. The new lines, which indicate hydrogen, and ionised oxygen, nitrogen, helium, and other elements, are, at first, absorption lines only, but broad, undisplaced bright lines soon appear on their red side, and gradually increase in strength through the second week following maximum brightness. A very curious feature of the spectrum is the existence of sharp, almost undisplaced sodium and ionised calcium lines. The second absorption spectrum gains in strength compared with the first, which is now dying out rather quickly. A broad, bright complex radiation at about λ4650 becomes very conspicuous and is visually the principal feature of the spectrum for several weeks or even months. It is a blend of enhanced lines of nitrogen, helium and carbon. About a fortnight after maximum, broad bright *nebular* lines make their appearance, particularly the unknown band at λ4363, characteristic of the *Pc* type of spectrum. As the star grows fainter, the nebular lines strengthen and become—after an interval varying from one nova to another—the most characteristic

PLATE XXXIII.



1. Spectrum of α Cygni.
2-12. Successive Spectra of Nova Cygni, 1920.

To face page 271.

features of the spectrum : we have, in fact, the spectrum of a planetary nebula. This may persist for many months, but, in practically all novæ that have been examined sufficiently, it gives place ultimately to a Wolf-Rayet spectrum, of class O. The final condition of a nova—spectroscopically, at least—seems to be that of a Wolf-Rayet star.

Plate XXXIII shows some of these changes as they occurred in the spectrum of Nova Cygni, discovered on August 20th, 1920, by Denning. Major Lockyer was able to photograph the spectrum before maximum brightness had been attained, with the result shown in No. 2 (No. 1 is a comparison spectrum of α Cygni). The dates on which the various spectra were obtained were : No. 2—August 22nd ; No. 3—August 22nd ; No. 4—August 24th ; No. 5—August 25th ; No. 6—August 26th ; No. 7—August 28th ; No. 8—September 10th ; No. 9—September 18th ; No. 10—September 23rd. In the spectra of particular novæ, there are, as would be expected, details of various kinds which give the history of each nova an interest of its own. Sometimes a third absorption spectrum appears ; the lines of all the spectra are frequently doubled ; the bright bands usually show a definite structure of some kind ; and so on. The general outline of the changes, however, seems to be almost universal.

The interpretation of this series of changes is exceedingly difficult. If Russell's, or any other rational theory of evolution be true, it must be acknowledged that a nova does not, like an embryo, recapitulate in its development the history of its race. It is unthinkable that the normal course of evolution is from a star giving an absorption spectrum, through an almost indescribable stage, to a planetary nebula, and finally to a Wolf-Rayet star. We have evidently to deal, in novæ, with something not at all akin to the regular course of cosmic evolution. In particular, the two sets of greatly displaced absorption spectra, and the very broad, undisplaced emission lines, are phenomena which demand a special explanation. The displacement of the absorption lines is generally ascribed to a Doppler effect, for the displacements seem invariably to be proportional to the wave-lengths of the lines. This is a characteristic, so far as we know, only of the Doppler effect and the possible relativity displacement, and the latter we know too little about to invoke here. But if it is a Doppler effect that is witnessed, the velocities involved must be enormous, amounting to thousands of miles per second.

Again, the great breadth of the emission lines must be significant, but significant of what? Perhaps of pressure, but more probably of a rapidly expanding shell of gas, in which the components of velocity in the line of sight would vary continuously from one part to another. Such a hypothesis

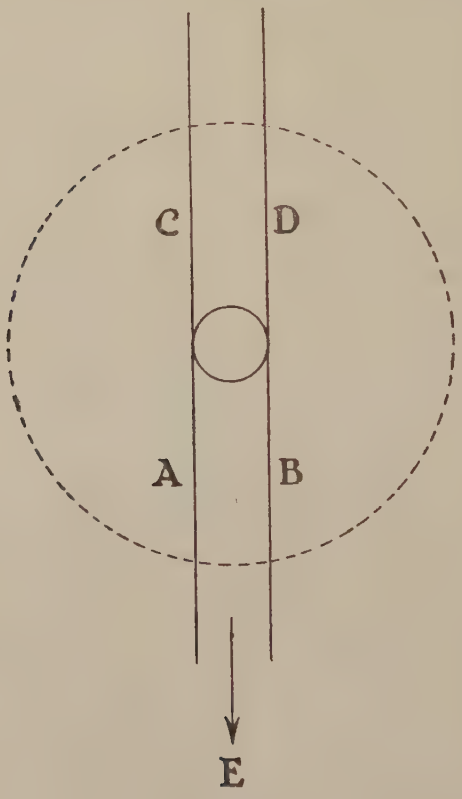


Fig. 29.

would explain why the lines are always broad, for it would make the effect independent of the direction of the line of sight. But then, why are there two sets of absorption lines, indicating different velocities? And why do they appear and disappear before their neighbouring radiations?

There is one explanation of a part of the spectroscopic

phenomena—at which we have just hinted—which is so simple and natural that it is hardly possible to doubt that it is true. If we imagine a bright star with a luminous atmosphere that is expanding outwards very rapidly in all directions, we can see that the spectrum of such a system would consist of absorption lines displaced to the violet, with adjacent broad, almost undisplaced emission lines—exactly what we find in the spectrum of a nova. For, in Fig. 29 let the inner circle represent the star, and the outer dotted circle the limit reached at any instant by the atmosphere in its outward expansion. At the distance of the Earth—supposed to be in the direction of the arrow *E*—the whole system would appear as a point, and its light would be analysed collectively in the spectroscope. The surface of the star itself would give a continuous spectrum, like that of the photosphere of the Sun. The part of the atmosphere between the two lines, *A* and *B*, would give an absorption spectrum with the lines displaced towards the violet, for that part of the atmosphere would be seen against the background of the stellar surface and the material in it would be moving rapidly towards the Earth. The rest of the atmosphere, however, would have no bright background, and would give its own bright emission lines. By Kirchhoff's principle, these emission lines would be the same as the lines in the absorption spectrum, so that every absorption line would have a bright attendant. Further, the bright lines would be very broad, with their centres almost undisplaced ; for the component of the atmospheric velocity in the line of sight would vary continuously from a swift approach just outside *A* and *B*, to an equally swift recession just outside *C* and *D*, and each region of the atmosphere would make its contribution, with its own peculiar displacement, to the complete emission line. We should therefore obtain exactly what we find in the spectrum of a nova.

If this were the only peculiarity of the nova spectrum, the only remaining problem would be to discover the cause of the expanding atmosphere. We might, indeed, be inclined to regard a nova as an irregular variable, in which the unknown disturbing agency had lost its soul for "one crowded hour of glorious life." But there are two, or perhaps three combined absorption and emission spectra of this kind, though only one supreme blaze of light. During the slow waning of the star, there are many fluctuations of magnitude, which are accompanied by changes of spectrum, but there is nothing

to suggest a second cataclysm like the first. If we suppose a second or third expanding shell of gas, we must account for its motion by some cause other than that which we assign to the first. Again, there are the stationary sodium and ionised calcium lines to be explained. Why should these lines be undisplaced; or, alternatively, why these alone? Leaving out of account the many other peculiarities of the spectrum, there is enough here to show that a simple expanding atmosphere is not sufficient to explain the observations.

Halm has proposed an expanding star, surrounded by rotating rings of luminous gases, as a possible source of the nova spectrum. Taking turbulent motion also into account, he deduces a number of the observed changes. Deslandres introduces convection currents and pressure effects, and attributes the change of spectral type to a dissociation, or breaking up, of elements into simpler ones, under disruptive conditions much more strenuous than any we have been able to produce on the Earth. The elements responsible for the second spectrum are lighter than those shown by the first, and this accords well with Deslandres's supposition. At the present time, however, as we have seen, change of spectral type is generally interpreted as a sign of change in the conditions of excitation rather than of varying chemical composition. Deslandres's suggestion, therefore, is less attractive than it might otherwise be, although it is very successful in avoiding the assumption of a second cataclysm. Stratton, who has made a special study of Nova Geminorum No. 2 (1912), points out that several of the spectroscopic facts can be explained by supposing that a number of successive jets, or streamers, issue from definite points on a rotating body. These jets would replace the expanding shell in Fig. 29, and the changes in the relative amounts of luminous gas respectively between and outside the lines *A* and *B*, would account for certain variations of intensity that are observed in the absorption and emission spectra. The phenomena in the later stages, however, necessitate the assumption of an expanding gaseous shell also. To explain the successive spectra, Stratton adopts the dissociation hypothesis of Deslandres.

It will be seen that the spectrum changes, taken alone, have not yet received a completely satisfactory interpretation. Apart altogether from the cause of the outburst of a nova, the occurrences in and around the body are baffling. It may

be, however, that some light will be thrown on them by an attempt to elucidate the cause of the whole phenomenon, taking into account the light changes and the high galactic concentration of novæ as well as their spectra. If we can make a reasonable proposition as to what a nova is, and why it exists, we might be able to work out the inevitable consequences of our hypothesis, and deduce spectrum changes which can be compared with those observed. Let us review some of the suggestions which have been made along these lines.

Most of the explanations of novæ involve the existence of two originally independent bodies. Such an assumption obviously simplifies the interpretation of the two complex spectra, on the one hand, and, on the other, of the range and rapidity of the light changes, which denote a cataclysm as shallow as it is violent. It is true that certain phenomena connected with novæ point to the spontaneous explosion of a single body from an unknown cause, and it is held by some that this is the true explanation. If so, however, it is little more than a confession of total ignorance, and we shall probably find it more profitable—at least until some *a priori* possibility of such an explosion can be shown to exist—to seek for a more fruitful hypothesis. A collision of some kind is inevitably suggested, but when the idea is worked out in detail, great difficulties arise. A collision between two dark stars might account for the sudden outburst, and if we could suppose the heat and light generated to be associated mainly with the stellar surfaces, the rapid decline might be explained. Or, as Bickerton urges, the rapidity of the light-changes would follow from the assumption that small portions of the colliding bodies coalesce into a " third body " which is the true nova. But this notion does not give a very convincing explanation of the fact that nearly all novæ show the same series of spectrum changes ; nor, indeed, does it naturally suggest any definite series of such changes at all. Moreover, from what we know of the density of distribution of the stars, a collision must be an exceedingly rare occurrence—far and away too infrequent to account for the number of novæ that have been detected. It is true that the stars in question are, by hypothesis, " dark " (*i.e.* their *apparent* magnitudes are too great for our telescopes to find them), and also that novæ appear to be confined to the galactic plane, where the stellar system is most populous ;

but, even so, it is hard to believe that collisions of stars can be frequent enough to account for all novæ. Lindemann has calculated that, to make this possible, there would have to be at least 4000 dark stars to every bright one. We cannot say definitely that there is not such a proportion, but we have no evidence that there is, and few are prepared to make the assumption in order to bolster up a hypothesis which, on other grounds, is seen to be improbable. Further, although novæ show a high galactic concentration, they prefer the borders of the Milky Way or the regions of dark lanes, where the star density is lower than it is in other parts of the galaxy. It seems to be necessary, therefore, to look for some other explanation of the existence of novæ.

There is another kind of collision which, in appearance at least, offers greater possibilities ; namely, the collision of a dark star with a nebula. This idea was put forward by Seeliger. It is suggested immediately by the not very deep but immediately obvious similarity between a nova and a shooting star, or meteorite. Shooting stars are phenomena of our own atmosphere. Small, dark bodies, travelling in space, encounter the Earth and enter its atmosphere at high speed. The sudden compression of the atmospheric gases which follows generates heat at an enormous rate, so that the intruder passes almost instantaneously into vapour, and ends its career in a trailing cloud of glory. Allowing for the difference in size and density between the meteorite and a star on the one hand, and the Earth's atmosphere and a nebula on the other, the appearance of a nova is remarkably like what we should expect from our experience of meteorites, if a star actually entered a dark nebula. The "meteoric" career of the nova, the double spectrum, the agreement between novæ, stars, and diffuse nebulæ in showing a high galactic concentration—these things, at least, are natural consequences of the hypothesis.

If a dark star entered a nebula at a high speed, we should expect the heat of resistance to be generated first at the surface of the star. It would raise the surface layers to a very high temperature, and therefore to very great brilliance, before the conductivity of the inner layers and radiation into space could induce it to bestow its favours with a less partial hand. But the star would rapidly slow down, for the heat would be formed at the expense of its motion. The source of heat supply would therefore diminish, and a time

would come when the brilliance of the star would reach its highest point and begin to fall through radiation and diffusion of temperature through the interior of the star. In our ignorance of the precise conditions of the encounter and of the bodies taking part in it, a few days appears as likely a period as any other for the rise to maximum brightness. The ultimate steady state of the star might be conceived as one in which the motion has been, in great part, converted into heat, whose radiation into space is balanced by whatever source of energy enables the stars in general to shine, without sensible weakening of their light, through the long ages of their existence.

The hypothesis is able, therefore, to give a sufficiently plausible account of the visual appearance of a nova. It is, moreover, not called upon to bring millions of supposititious bodies into existence, for the vast extent of a nebula would make the chances of encounter with a star incalculably greater than the chances that two dark stars would collide. Further, the galactic plane is the most likely place for such a collision to occur. In all these details, the explanation is natural and complete. There remains only the greatest difficulty of all—the peculiarities of the spectrum. It cannot be said that the success of the hypothesis here is altogether complete, but on the other hand, the explanation it offers is the best that has yet been given. Supposing a star to rush into a nebula—conceived, in the absence of definite knowledge, as a cloud of very fine particles embedded in a mass of gas—we might imagine the following series of events to occur.

The original sudden heating of the stellar surface would naturally yield a continuous spectrum, for although a large part of the surface would probably be vaporised, the vapour would at first be very dense. The extreme brightness attained by the star would repel the small particles of the nebula, which, both by radiation from the star and by the friction arising from their rapid motion, would soon become heated enough to emit, and therefore absorb, light of their own. The retreating nebula would therefore be the expanding shell of gas represented in Fig. 29. In this way we should get the first absorption spectrum, gradually increasing in strength, and later, when the particles had retreated far enough and become hot enough for their light to be perceived independently, the broad bright lines would appear. It is hardly possible to predict what type of spectrum the nebula

would give. Considering the extremely low pressures that must exist in nebulæ, however, it is not a matter for surprise that we obtain enhanced lines. During the outward motion of the nebula, the radiation pressure would decrease fairly rapidly, on account both of the increasing distance of the particles from the luminous star and of the waning of brightness of the star itself. The particles, therefore, subject to the resistance of the more outlying nebular regions into which they would be moving, would lose speed. Accordingly, we find the first absorption spectrum moving slowly to the red until it is lost in the broad bright lines of the second spectrum.

So far the explanation seems to be satisfactory. There is now, however, the second spectrum to be explained, and we have, in fact, the changes taking place in the heated star, which we have so far ignored, waiting to be called upon to explain it. Whether or not they can do so satisfactorily is perhaps an open question. With our present knowledge and experience, it is hardly possible to predict what will happen to a huge body, in an unknown state, when its surface is heated to an enormous temperature with extreme suddenness. Something must happen, however, and it is as likely a hypothesis as any other that the star will expand rapidly outwards.[1] If so,—and the assumption, it should be noticed, is, in one form or another, a part of all existing explanations of the nova spectrum—then the expanding shell of gas will give us our second absorption and emission spectrum, following the same general course as the first in the circumstances of its appearance. It might be remarked that it seems almost inevitable that the result of bombardment by the nebular particles would be to destroy the surface of discontinuity between the interior and the atmosphere of the star; *i.e.* the photosphere. The boundary of the star could scarcely be in any but an extremely unstable state, and it is difficult to imagine what it could do but expand outwards. Again we cannot predict what spectrum should appear, but we can say that, in all probability, it will be of an earlier type than the first. For the temperature of the actual stellar gases must be considerably higher than that of a nebula heated by radiation and friction, and, further, the gases will expand into the almost absolute vacuum left by the flying nebula, so that their pressure will rapidly fall to a

[1] See page 396.

very low value. As a matter of fact, we get a *B*-type spectrum—a stage earlier than the original one of type *A*. With regard to the velocity of the outrush, though its actual magnitude is beyond calculation we can once more say that it should be greater than that of the nebular particles. The new outflow encounters no resisting medium; the atoms producing the observed lines are lighter than those of the nebula; the radiation pressure at equal distances is greater in the second outrush, when the radiation is more intense; and there might, in addition, be an electrical repulsion of the ionised emitting atoms, of the kind imagined by Fowler to operate in the solar chromosphere (p. 196). All these things, added to the original impetus responsible for the outrush itself, could hardly fail to give the second outburst a higher velocity than the first. It is found, in fact, to be about twice as great, if the displacements of the spectrum lines are due entirely to Doppler effects.

What should happen after this is pure conjecture. The star is getting fainter, and the outgoing nebula and stellar gases must be getting cooler. If matter continues to be poured out from the star, as observations seem to indicate, the maintenance of the second absorption spectrum is naturally explained. The light of the original outpourings would be expected to fade away and play no further part in the phenomena, and, until lately, no one would have supposed that it could do anything else. In view of recent ideas of the character of the luminosity of nebulæ, however, the circumstances are extremely suggestive. We have a central luminous star, surrounded, at a respectful distance, by a shell of nebulous matter. Now this is just the state of affairs in planetary nebulæ. There we have, as a rule, a central star with a surrounding nebula, but the nebula is luminous. This seems to be an important difference, but the interesting point is that astronomers are now tending to the conclusion that nebulæ shine, not by virtue of high temperature, but by some kind of fluorescence or photo-electric action, excited by the associated stars. This idea, which we shall discuss in the next chapter, is suggested by the almost, if not quite, universal association of the luminous gaseous nebulæ with bright stars. Looking at this idea from our present point of view, we must regard it as highly significant that the spectrum of a nova, after the first set of absorption and emission lines has faded away, begins to show the characteristics of a nebular spectrum;

The disappearance of the *A*-type spectrum that we assigned to a hypothetical nebula, is succeeded by a *P*-type spectrum which elsewhere is found only in nebulæ. It is hard to avoid the suggestion that the material source of the two spectra is the same, and that the difference in appearance of the spectra arises from the difference between temperature radiation and fluorescent or other radiation. If this is so, it must have a very important bearing on the nature of nebulæ and the nebular radiations.

The next stage in the spectrum—the disappearance of the nebular lines and the development of Wolf-Rayet bands—would naturally be associated with the weakening of the stellar light so that it could no longer induce the peculiar glow in the surrounding nebula. Why the star should assume Wolf-Rayet characteristics is apparently inexplicable, but then, Wolf-Rayet radiation in general is inexplicable. If, however, as we have suggested, the star in encountering the nebula loses its surface of discontinuity between atmosphere and interior, and the breach is not repaired, a spectrum similar to that of a Wolf-Rayet star—namely, one with bright, broad radiations on a fainter continuous background —would be expected. This, at least, we know, that the stellar nucleus of a planetary nebula is almost invariably a Wolf-Rayet star, and, further, whenever we find a Wolf-Rayet star, the chances are that it will be involved in nebulosity. Wolf-Rayet stars and nebulæ go together, and when we find, in a nova, a nebular spectrum succeeded by a Wolf-Rayet spectrum, it is almost impossible to doubt that, whether or not a collision of a star with a nebula was the original cause of the outburst, both a star and a nebula are in some way involved in the succeeding changes. In one nova —Nova Persei No. 2, of 1901—there was evidence of this, almost amounting to proof. Some time after the appearance of the nova, a faint nebulosity seemed to be spreading out from the star. The rate of spreading, if the appearance was due to a diffusion of actual matter, indicated an improbably large parallax on any reasonable assumption of the velocity of the matter. But, assuming that what was seen was an illumination of a relatively stationary nebula by light originating in the star, a satisfactory parallax was calculated, and it is generally believed that this is what was happening. The illuminated area would then have been growing with the speed of light.

The principal phenomena of a nova can therefore be accounted for, though not explained, by the hypothesis that they arise from a collision between a star and a nebula. In the brief account we have given, many details have, of course, been ignored. Some of them must doubtless depend on the particular circumstances of the collision—the physical states and dimensions of the star and nebula, the relative speed of the collision, its direction with respect to the line of sight, and so on. These things must be left until, with increased powers of observation and knowledge of physical processes, we are able to deal with them. One important fact, however, demands attention, because, occurring invariably as it does, it must belong to the general character of a nova and not to the accidental circumstances of a particular outburst. We refer to the "fixed" absorption lines in the spectrum—the *H* and *K* lines of ionised calcium and the *D* lines of sodium—which, amidst the otherwise universal displacement of absorption lines, remain steadfast at their posts.

This curious anomaly is not an obvious consequence of any of the conditions we have pictured. Having elected to ascribe the displacements to a Doppler effect, we must, of course, regard the stationary lines as the product of a substance which has little or no component of velocity in the line of sight; and the fact that the lines are absorption lines compels us to place that substance between the star and the Earth—unless, indeed, the lines originate in an atmosphere which remains faithful to the star when the bulk of its gases are deserting it. It may be, of course, that we are wrong in concluding that there is any appreciable Doppler effect at all; perhaps the observed displacements arise from some different, possibly quite unknown, cause. Adams has made an observation which it is difficult to reconcile with the Doppler explanation. He finds that, in the spectra of four novæ, all the relative displacements [1] of the lines which occur fall into three groups, the values of the displacements in these groups being, respectively, one, two and three times a common factor. It is hard to imagine any physical circumstances which would account for so simple a relation between the

[1] By "relative displacement" is meant the displacement of a line divided by the wave-length $\left(\frac{d\lambda}{\lambda}\right)$. In a Doppler effect, this quantity is constant (p. 35), so long as the relative velocity of the source and the observer is constant: it is constant also in each of the component spectra of a nova.

velocities of expanding gases, not only in one nova, but in four taken together. If the displacements are really due to some cause other than the Doppler effect, then the fixed lines might be simply particular cases of the general rule, in which the multiple of the common displacement factor is zero. But this supposition leads us no further. If the displacements are not due to velocity, we do not know what they mean. If progress is to be made, we must regard them as Doppler effects, and look for some special reason why the sodium and ionised calcium lines behave in a special way.

The problem is complicated by the fact that precisely the same phenomenon occurs in stars which are not novæ. For some time it has been known that these same lines—the *H* and *K* lines of ionised calcium and the *D* (and sometimes other) lines of sodium—appear exceptionally sharp and remain fixed in a number of spectra of *O* and *B* type ; *e.g.*, the spectra of the stars, δ Orionis, ο Persei, β Scorpii. As a rule, the stars are spectroscopic binaries—that is, the bulk of the lines oscillate to and fro, indicating a variable radial velocity—but some stars which are apparently single have the same characteristic. The fixed lines are not always undisplaced. They may show a Doppler effect arising from the solar motion, or even a still greater displacement. They do not, however, oscillate with the majority of the lines. In some stars, again (*e.g.*, 9 Camelopardalis), they show an oscillation, but one having a smaller range than that of the other lines. For some reason, these particular lines seem to be capable of appearing strongly under conditions which give them a greater inertia than that of the other lines.

No completely satisfactory explanation of these facts has been given. It has been suggested that the lines in question are telluric lines, but this idea can now be definitely disposed of. Non-luminous calcium vapour would not absorb ionised lines alone unless there were present some agency capable of bringing about complete ionisation at a low temperature. We know of no such agency in the air. Moreover, the displacements of the lines reflect the motion of the Earth, which telluric lines would not do. Again, the lines have been attributed to one component of a binary star which is supposed to be much more massive than its companion, and therefore to have a much smaller orbital velocity. This suggestion also is inadequate ; the phenomenon is probably not confined to binary stars, and the necessary disparity

between the masses of the components is very unlikely to occur. Perhaps the most helpful suggestion is that the lines are due to absorption by clouds of vapour between the Earth and the star. The clouds must be supposed to be not very distant from the star, for in some cases their oscillation shows that they share, to a small extent, in the variation of radial velocity. Radiations of some kind from the star (cf. p. 220), might produce the ionisation of the calcium atoms. The restriction of the fixed lines to *O* and *B* stars would be expected on this hypothesis, for these stars are precisely the ones which are found in association with gaseous nebulæ, and the nebulæ might be supposed to constitute the clouds. On the other hand, Jeans suggests that the clouds are distant atmospheres of the stars themselves, and not independent nebulæ. His theoretical researches indicate that a star, in its very early stages as a binary, should be attended by such an atmosphere. The suggestion gives a possible explanation, though it is not clear why sodium and ionised calcium are the particular substances responsible for the lines which behave in this way, nor—a more serious objection—has *radiation* of the lines in question from the supposed clouds ever been observed.

We may assure ourselves, however, that since stationary lines appear in the spectra of ordinary stars, which show no other characteristics of novæ, their explanation is not necessarily to be found in the course of events which gives to a nova its intrinsic peculiarities. The apparent failure of Seeliger's hypothesis to require the stationary lines as an inevitable consequence does not mean that the hypothesis must be abandoned or even modified. It simply means that, if the hypothesis is true, the events it contemplates must be accompanied by some other process which, in novæ as in other stars, produces the stationary lines. The discovery of that process would make the explanation of a nova more complete, but would not necessarily alter existing ideas in the least.

Three other points deserve mention in connection with the subject of novæ. First, in strong support of the hypothesis of an expanding stellar atmosphere, it has been observed that in their later stages, Nova Persei No. 2 (1901), and Nova Aquilæ No. 3 (1918), show discs of finite size in the telescope. The discs, in fact, have been observed to grow larger. Nova Aquilæ, which measured 0″.65 in October, 1918, had, by

June, 1920, grown to 3″.69. On any reasonable hypothesis as to the parallaxes of the stars, this rate of growth precludes the explanation of the appearance as an illumination of surrounding matter by spreading light, and, moreover, in Nova Persei, as we have seen, such a spreading was observed long ago to take place at a far greater rate. We can hardly doubt, therefore, that, in these stars, at least, there is a rapid outward movement of luminous matter.

The second point concerns an exception to the ordinary phenomena of novæ. The star, *T* Coronæ, which we have mentioned before, was, prior to 1866, a star of apparent magnitude 9.5, and was known under the designation of B.D.+26° 2765. In 1866, however, it suddenly flared up into a second magnitude star, and then slowly died down, until to-day it appears to be in the same condition as it was before its outburst. So far as light changes are concerned, therefore, it is a typical nova. In other respects, however, it has the character of a long-period or an irregular variable. It is a giant star of type *M*, and it lies much farther from the Milky Way than do novæ in general. It appears to demand a special explanation which has not so far been forthcoming. In striking contrast to this is the star *P* Cygni, which was a nova so long ago as the year 1600, and now shows a spectrum like that of a nova in the early stages. What its spectroscopic history has been is, of course, for the most part unknown. In the absence of this knowledge, it is probably useless to speculate on its character.

Thirdly, Lundmark has recently made some interesting observations concerning novæ. He points out that they show a conspicuous preference for the Sagittarius-Scorpio region of the Milky Way, and, further, that a nova has never been observed within the region of the Milky Way between galactic longitudes 187° and 255°—containing the constellations Argo, Canis Major and Monoceros. The latter region we have already (p. 97) had occasion to mention as the direction towards which the fastest stars are moving: it appears to have some special significance in the structure of our system.

Collecting all the available data with regard to the parallaxes of novæ, Lundmark finds that, at maximum brightness, these stars have, on the average, an absolute magnitude of −6.2. They would appear, therefore, to be brighter at this stage than any other class of star. He states that novæ

arise from both giant and dwarf stars. The only giant star, however, which is known to have become a nova is *T* Coronæ —a very abnormal example. All typical novæ whose parallaxes are known were dwarfs before the outburst, so that it seems more reasonable to assume tentatively that only dwarf stars become novæ. Lundmark calls attention also to an estimate made by Bailey that, each year, more than nine novæ occur which become brighter than the 9th apparent magnitude at maximum. If this is so, and if the number of stars which appear as novæ more than once in their lives is so small as to be negligible, then, in a few million years, there will be as many "old novæ" as there are stars in the whole sky brighter than the 17th apparent magnitude. Supposing, now, that conditions in the stellar universe have not changed greatly within the last few million years, it follows that almost every star in the sky has been, at some time or other, a nova. Lundmark suggests that, in the future, novæ may come to be regarded, not as accidents, but as representatives of a stage through which the majority of the stars must pass.

This notion establishes a very suggestive point of view, but it involves too many gratuitous assumptions to be considered as a serious hypothesis. It would be exceedingly difficult to reconcile it with the known facts of stellar spectra, which indicate, as we have seen, a course of development in which there is no room for the catastrophic changes associated with the career of novæ. The link between novæ and the Harvard sequence connects Wolf-Rayet stars to the stars of class *B*, and it seems hardly possible to accept it as a part of the chain of events by which most of the stars are characterised. Wolf-Rayet stars, even on account of their scarcity alone, must be regarded as exceptional bodies, and though their spectra pass continuously into those of class *B*, the sequence probably represents a continuity of physical conditions rather than of the causes which give rise to the conditions. We pass on now to a consideration of Wolf-Rayet stars, as a natural epilogue to the study of novæ.

Wolf-Rayet Stars.—The Wolf-Rayet stars, of which between 100 and 200 are known, are among the most interesting of celestial bodies. They are apparently very faint—very few are visible to the naked eye, and these, with the exception of the type star, γ Argus (or γ Velorum), can only

just be seen—but that is because they are very distant ; it does not mean that they are absolutely faint. They are highly concentrated to the Galaxy—a characteristic which, as we have already explained, might be an effect of their great distances. They are, as a rule, intrinsically very bright, but occasionally an abnormally faint one is found. Their spectral type, as we know, can be traced continuously from that of the *B* stars, and there are other characteristics also in which they continue the tendency of the Harvard sequence beyond the *B* stage. Thus, their temperatures are higher, their masses appear to be greater (though our knowledge of the masses of true Wolf-Rayet stars is very scanty), and the velocities of some of them are smaller than the corresponding qualities in the *B* stars. Their spectrum lines, which are bright on a faint continuous background, are very broad and diffuse, with one notable exception, the star D.M.+30° 3639. This star, which has received a great deal of attention, appears to be surrounded by a vast atmosphere of hydrogen, which Wright considers to be nebulous.

The great interest inspired by these bodies is concentrated in the study of two problems : first—why, when the majority of stellar spectra consist chiefly or entirely of absorption lines, do these stars give spectra of bright lines ? second—what is the relation of Wolf-Rayet stars to the normal types ? These questions are interdependent, and neither of them has yet received a complete answer.

The relation of Wolf-Rayet stars to the normal types cannot be finally settled until we know what the state of Wolf-Rayet stars really is. There are, at least, two clear alternatives, each of which has strong points in its favour and is faced by, perhaps, equally strong objections. The Wolf-Rayet condition may represent a stage in the normal evolution of some stars, running smoothly into or out of the *B* condition, or it may be the Indian summer of a nova, the tranquil eventide of a few strenuous lives.

The chief evidence for the first alternative is, of course, the unbroken sequence, not only of spectral type, but of several other characteristics also, from the *O* to the *B* state. Temperature, colour, velocity, galactic concentration, possibly mass—all exhibit the sequence, which appears to afford an argument for including the *O* stars in the evolutionary scheme as strong as the argument for stellar evolution itself. We can suppose that stars still more massive than the average

B star go beyond the *B* stage into the *O* before starting on their return journey. The fact that there are very few of them would be naturally explained by supposing that stars sufficiently massive are very rare objects; this is quite in keeping with—and, in fact, is a necessary requirement of—Eddington's theory of stellar equilibrium. The difficulties facing this hypothesis are two-fold. First, if it be true, it is hard to explain the change from absorption to broad emission spectrum lines: second, we know that novæ develop into Wolf-Rayet stars along lines totally different from those of the accepted course of evolution, and the close association between novæ, Wolf-Rayet stars, and planetary nebulæ seems to suggest that these bodies are partners in irregular proceedings rather than law-abiding subjects. These objections are serious, and must be given careful attention.

With regard to the bright lines, there are at least two possibilities of explanation, neither of which is beyond suspicion. We have seen that, in the process of condensation, a star would reach a maximum temperature, first at the centre and later in the outer layers, so that, about the time of its maximum, the centre would be cooling while the outer layers would be getting hotter. The period between attainment of maximum temperature at the centre and near the exterior of the star would be greater, the greater the mass of the star. It might therefore be possible that, in a very massive star, the temperature in an outer region, where the density would be low enough to permit radiation of a line spectrum, would be higher than the effective temperature corresponding to the light emerging from the interior. If so, then we should see a bright line spectrum on a fainter continuous background. Broad lines would be expected from a probably thick layer exhibiting a large range of temperature and pressure. In circumstances such as these, the line spectrum would represent a higher temperature than the continuous spectrum, whereas, in the dark line stars, it represents a lower temperature. Since the effective temperatures deduced from the continuous spectra are higher for the *O* than for the *B* stars, the temperatures at which the lines are produced would be much higher in the *O* stars, and might account for the production of lines of which, under the gentler stimuli possible in the laboratory, we have obtained no knowledge.

The second possible explanation of the bright lines is that

they are induced radiations, created, not by high temperature in the source of radiation, but by the action of light or electrified particles, according to laws which are at present very imperfectly understood. We have already referred to this kind of luminosity as the probable origin of the spectra of planetary nebulæ, and we have mentioned that planetary nebulæ are frequently associated with Wolf-Rayet stars. It seems as if these stars have a genius for persuading neighbouring matter to radiate without the incentive of a laborious heating to incandescence, and we must therefore admit the possibility that the Wolf-Rayet lines are produced by such a process. The broadness of the lines is not so naturally explained on this hypothesis as it was on the former one: it seems more likely to be a Doppler effect of some kind. The explanation we adopted for novæ, however, would hardly be available here. It is almost impossible to imagine that outward expansion can go on continuously in a permanent star. If such a thing should happen, the star would either show an ever-increasing disc—which it does not do—or else the flying matter would be ceasing to radiate at a certain distance from the star. If, by some unknown process, a star could fritter away its matter, changing its heat back into work in this way, we could imagine a continuous evolution of the extremely massive stars past the *B* into the *O* stage, from which they pass into dark, diffuse nebulæ. Such a process, however, is beyond dynamical or thermodynamical sanction, and although that is not a final argument against its possibility, the hypothesis is an impracticable one in the present state of our knowledge. If the broadness of the Wolf-Rayet lines is due to velocity displacements, it is to turbulent motion that we must look for the most likely source of the velocity—a type of motion, moreover, that would be expected at extremely high temperatures.

But the chief interest of the induced luminosity idea is that it leads us directly to the alternative explanation of Wolf-Rayet stars—that they are the last stages of novæ. We know that novæ do develop into Wolf-Rayet stars: it has been suggested that *all* Wolf-Rayet stars were once novæ. This hypothesis runs directly counter to the view that they are normal products of stellar evolution. If we adopt it, the problems we have to face are quite different from those we have just considered. The nature of the spectrum no longer holds the chief interest for us, for we have evidence that novæ

can yield a Wolf-Rayet spectrum. It is therefore irrelevant, though extremely interesting, to discuss the meaning, and establish the consistency with our assumption, of every spectral detail. What we have to do is to explain why, if Wolf-Rayet stars invariably develop from novæ, they continue the sequence of the main Harvard types in so many characteristics.

We must observe, however, that if Wolf-Rayet stars excite non-temperature radiation—and, whatever the Wolf-Rayet lines themselves might be, a number of the stars probably stimulate this radiation from planetary nebulæ—then the grounds on which the sequence of characteristics is established are open to grave question. The temperature, for example, is deduced from the distribution of energy in the spectrum; but if the energy of the continuous spectrum is partly used up in producing non-temperature radiation, and if, as is almost certain, some wave-lengths are called upon more than others for this work, the energy distribution in the resulting spectrum will no longer indicate the effective temperature of the star. Nay, apart altogether from the assumption of non-temperature radiation, if the radiation from a Wolf-Rayet star originates chiefly at the surface, it is, to say the least, unlikely that its energy distribution will be comparable with that of the composite radiation finally emerging from a star which is hottest at the centre. If all Wolf-Rayet stars are developed novæ, it seems that we must regard their temperatures as undetermined.

Again, the small velocities of Wolf-Rayet stars might occur just as naturally if the stars were once novæ as it would if they are products of the ordinary course of stellar evolution. If a nova arises from the passage of a star through a nebula, then the resistance of the nebula would necessarily cause the star to slow down. So far as we can judge, diffuse nebulæ are almost at rest in space: novæ, therefore, would be brought nearly to rest also. The circumstances would prohibit any correlation between mass and velocity, and, so far as we can tell, a nova might have almost any mass at all—at any rate, novæ might show a very large range of mass. The small amount of evidence we possess indicates that Wolf-Rayet stars have high masses, but it is quite inadequate as a basis for generalisation. It is confined mainly to the later divisions of the spectral class, where absorption lines predominate. Moreover, the evidence is based on the doubling

or oscillation of the spectrum lines, which is held to arise entirely from the orbital motion of a spectroscopic binary. If, however, the lines are produced in neighbouring matter, and not in the star itself, this interpretation must be modified.

The apparent continuity between *B* and *O* stars might therefore be illusory. The gradual transition of spectral type denotes, perhaps, a continuity of certain physical conditions, but the existence of giant and dwarf stars alone is sufficient to show that this by no means necessitates a *nexus* in the evolutionary chain. It is undoubtedly remarkable that the continuity should appear to embrace so many characteristics, and should break down completely in none, and the influence of this fact might well prove too powerful for an argument which has to employ a separate weapon for each direction of attack. It must be remarked, however, that in respect to absolute magnitude, there is a partial discontinuity. Some Wolf-Rayet stars have exceptionally low absolute magnitudes—a fact difficult to reconcile with abnormally large mass, but not at all inconsistent with the nova hypothesis. It might appear, indeed, that feeble luminosity would be incapable of exciting sympathetic vibrations in cold matter, but certain photo-electric phenomena suggest that the power to do this might depend more on the wave-length than on the intensity of the light. If Wolf-Rayet stars shine mainly on the surface, they are probably capable of discharging very short and penetrating radiations, as well as high-speed corpuscles, which, in an ordinary star, would be largely absorbed by the cooler outer layers. Moreover, if the nova hypothesis be adopted, the low absolute brightness of the exceptional Wolf-Rayet stars might be assigned to small surface area, and not at all to deficient brightness per unit area. The power to excite non-temperature radiation would, on this assumption also, be left untouched.

On the whole, the question is still in a very unsettled state. It may be that Wolf-Rayet stars are not a homogeneous group—that some of them are normal stars, while others are the relics of past novæ. If so, the likeness between the two classes must be temporary. They must be destined to pursue perhaps entirely different courses in the future, and what lies in store for those which once were novæ we can hardly conjecture. Enough has been said, however, to show why these

bodies have a special interest for astrophysicists. We pass on now to other exceptional types of star.

Special Types of Red Star.—At the other end of the Harvard sequence we find the small classes, *R* and *N*; and here the uncertainty with regard to the relation of the stars to the main sequence is not so acute. The relation is not at all obvious, but there is fairly convincing evidence, as Curtiss has shown, that these two classes lie on a branch leaving the principal chain after the *G* class. Thus, while the great majority of later type spectra follow the course *G*, *K*, *M*, a certain number show the sequence *G*, *R*, *N*. The chief difference between the trunk and the main branch of the spectral succession is that while, in the former, the bands of titanium oxide are pre-eminent, in the latter there is a predominance of bands due to carbon and its compounds. The absorption *lines* are almost the same in both sequences, and the resemblance is carried still further by the continuous tendency towards long-period or irregular variability, and the increasing part played by bright lines in the spectra, as we advance along either sequence. We have already noticed these two characteristics in the main succession of types. We must now record that bright lines appear in both *R* and *N* spectra, but are stronger and more numerous in the latter; and further, that while about 8 per cent. of *R* stars are variable, only two or three invariable *N* stars are known. It appears that the distinction between the two sequences is not so fundamental as the spectra would suggest. A band spectrum shows an exterior semblance which altogether belies its real importance. The spectra of *M* and *N* stars, though apparently quite dissimilar, may, in reality, be differentiated only by very slight differences in physical conditions or chemical composition in the stars—differences which, when the stars are at the age at which they yield earlier type spectra without bands, produce no perceptible distinguishing characteristics.

Stars of class *R* are apparently very faint—too faint to be visible to the naked eye. Those which are known are mostly giants, but it does not follow that there are actually more giants than dwarfs. Dwarf stars of late type must be very faint objects, and would be detected in much smaller numbers than the giants. The radial velocities of *R*-type stars, according to Sanford, are extraordinarily high; the mean value, freed from the solar motion, is about 31 miles per second.

The velocities show no uniformity of direction or magnitude, so that the stars appear to have no systematic motion, as a class, with respect to the stellar universe. This fact, taken in conjunction with the high values of the velocities, suggests that their masses are small. The galactic concentration of *R* stars is less than that of the stars as a whole. In all these characteristics, the stars of class *R* show a striking resemblance to the long-period variables of class *M*—another indication that there is no essential difference between the two spectral sequences.

Pickering, in an early account of *R*-type spectra, described them as *O*-type spectra reversed—that is, with dark lines and bands instead of bright ones. Such a resemblance would be most remarkable, because, while *O* stars appear to be the hottest of bodies, the temperatures of stars in class *R* are below those of stars in class *M*. It would suggest that the Wolf-Rayet stars are not so hot as the temperature measurements indicate, and give considerable support to the hypothesis we have already touched upon, that the continuous spectra of Wolf-Rayet stars, having devoted some of their light to the creation of non-temperature radiation, are no longer able to declare their temperatures in the language employed by perfect radiators. Rufus, however, who has examined a number of *R*-type spectra with a slit spectrograph, declares that the resemblance between *R*- and *O*-type spectra is an illusion. He considers that there is no evidence of any physical relationship between the two types of star. Prominent bands appearing in the two spectra are found to be in slightly different positions, and although *R* spectra contain bright lines, these are, in general, different from the bright lines shown by the Wolf-Rayet stars.

Progression through the subdivisions of class *R* is marked by a steady disappearance of light of shorter wave-lengths, and leads continuously into class *N*. The stars of class *N*, though in many ways the obvious neighbours of the *R* stars, show also some characteristics of their own. The increasing strength and numbers of bright lines, and the prevalence of variability, have already been mentioned. The temperatures, according to the Potsdam measurements, are between those of classes *M* and *R*. It must be remembered, however, that, when strong absorption bands are present in the spectra, temperature measurements are subject to considerable uncertainty. The decreasing extension of the spectrum at

the violet end as we proceed from the *R* to the *N* stars suggests that the temperatures of the latter are really lower than those of the former. The values of the colour indices for the respective groups support this suggestion. The *N* stars are decidedly redder than the *R* stars; the colour indices of some of them are as high as +4. It is therefore probable that the temperature sequence is continuous along the *G*, *R*, *N* branch of the spectral succession.

The average radial velocity of 25 *N* stars has been found by Moore to be about 11¼ miles per second—considerably smaller than Sanford's value for the *R* stars, but pointing to higher velocities than those of *M* stars. The proper motions, however, are exceedingly small, indicating that stars of class *N* are very remote. Their galactic concentration is very high, though not so high as that of the Wolf-Rayet stars. This again suggests great distances. Three distinct lines of argument indicate that the *N* stars have greater absolute brightness than *B* stars. They must therefore be supergiants, and their apparent faintness is only another consequence of their remoteness. Again it should be remarked that the apparent absence of dwarf *N* stars may have no real significance. The very low luminosity which would almost certainly be possessed by these bodies would make it quite impossible for any of them to be detected at distances comparable with those of the known giants.

Shane has made a special study of *N* stars, and has identified many of the absorption lines in the spectra. These lines are so crowded as to make it doubtful, at first, whether the appearance of bright lines is due to actual line emission or to narrow regions of unabsorbed continuous spectrum which show up strongly by comparison with the neighbouring dark lines. Shane gives evidence that the bright lines are actually due to gaseous emission, though none but those of hydrogen can yet be identified. He inclines to the view that, above the heavy and cool reversing layer which is responsible for the absorption lines, there are gases emitting the bright lines, and that, surrounding the whole, there is an atmosphere composed in part, at least, of carbon vapour. Possibly the higher regions of this atmosphere condense in a cloudy form, and, by selective absorption of the shorter wave-lengths of the internal light, give to the stars their characteristic ruddy colour. Shane anticipates that, as the condensation increases, the stars will grow fainter and fainter, and finally become

totally extinct as luminous bodies. It is difficult to reconcile this idea with the fact that *N* stars are giants and have exceedingly high total luminosity. The hypothesis would be less open to objection if applied to dwarf stars, but we have no evidence that dwarf *N* stars exist.

It is not at all clear yet why a comparatively small number of bodies should begin their stellar life in class *N* rather than in class *M*. As we have pointed out, the difference between the two classes is probably not very deep, and is given a false emphasis by the predominance of bands in the spectra. When, in class *G*, the bands disappear, the two sequences become indistinguishable. The higher velocities found in the *G*, *R*, *N* branch suggest that the stars there are less massive than their neighbours. Little weight, however, can be given to the suggestion, for the evidence for equipartition of energy lies mainly among the dwarf stars, and is particularly questionable when applied to giants at the very beginning of their lives. It would be very interesting to know whether stars ever pass through the *R* and *N* classes in their old age. Only the actual detection of dwarfs would settle the question, for their non-appearance we have shown to be ambiguous. At present, however, the existence of two tribes of young giants helps us to understand more clearly what are the deeper-lying characterisitics of early stellar life. Variability and the production of bright spectrum lines (especially those of hydrogen) seem to be fundamental: the superficial spectral characteristics—such as the assertiveness of titanium oxide—belong, on the other hand, to the inessential qualities of the stars. It may be that there is here an indication that long-period and irregular variability are closely associated with outbursts of glowing gases—a suggestion already put forward to explain the vagaries of Mira. Possibly, in the youngest stars we have a riot of the flames which we find much sobered in the solar prominences. At all events, an interesting line of investigation is revealed by the detection of qualities common to stars of different spectral types at comparable periods of their lives.

Of particular interest from this point of view is the study of the newly-isolated class *S*, which appears to form a third branch, separating from the main sequence between *K* and *M*. Almost the only knowledge we have of the properties of this class comes from a recent preliminary investigation by Merrill. Adams and Kohlschutter's criterion for absolute

magnitude (p. 81) indicates that the stars are giants, with a luminosity comparable with that of the giants of classes *K* and *M*. The stars are apparently faint, however, so that, like *R* and *N* stars, they are probably very distant. The prevalence of variability and of bright spectrum lines characterises class *S* just as it does the parallel sequences—supporting the view that these qualities belong to the essential nature of very young stars. The origin of the bright lines, however, is surprising. Apart from the hydrogen lines, the most prominent of them arise from ionised iron. The presence of bright, enhanced lines among stars of the latest spectral types is most unexpected, and creates an interesting problem for the future. The stars are red: the colour indices of the variables, which form a large majority of the present list, seem to be about the same as those of the *Md* variables—about 1.8 magnitudes.

A special interest belongs to the giant stars at the bottom of the Harvard sequence because, according to our present ideas, they represent the earliest ages of stellar life. We believe that, before a quantity of matter becomes a star, assuming a globular form and emitting light, it exists as a nebula of some kind. We have examples of diffuse nebulæ—shapeless and enormously distended—and we have giant stars of inconceivably low density. But between the two there is a gap. Nature seems at present to have left no trace of the process by which, if by any, a diffuse nebula generates a giant star. Planetary nebulæ would appear partially to fill the gap, but the evidence is strong that they do not belong to the normal evolutionary process at all. The only way to solve the problem is to work, as far as possible, onwards from the nebulæ and backwards from the young giant stars. Every fact we can discover about these bodies has therefore more than an ordinary interest, and may be the means ultimately of filling the greatest gap in the evolutionary chain.

Stars with the c Characteristic.—Among the many stars with highly interesting peculiarities in their spectra, there is one group that deserves special mention. In the earliest days of the Harvard spectral classification, before the present nomenclature was introduced, the spectra were arranged in a sequence, more or less analogous to the present one, by Miss Maury. Near the beginning of her arrangement—at a stage

corresponding to the present spectral class *A*—she divided the spectra into three parallel sequences, distinguished from one another by the degree of sharpness of the lines, and merging into a single chain among the later types. The sequences were designated *a*, *b*, and *c*, and the *c* stars in particular, though not specifically recognised in the present Harvard classification, are now generally acknowledged to possess characteristics which distinguish them from the other stars of their type. Their spectral lines are exceptionally sharp and narrow, and the enhanced lines are remarkably strong. They belong, as we have said, chiefly to class *A*, but since, as a class, they have no place of their own in the modern sequence, it is usual to speak of "stars with the *c* characteristic" rather than of "*c* class stars."

Stars with the *c* characterisic appear to have a greater intrinsic luminosity than any other class of star—with the possible exception of novæ at their maximum. For the sharpness of their spectrum lines and the great strength of the enhanced lines, they are probably indebted to extensive atmospheres of very low density. Their radial velocities are rather higher than the average for stars of their spectral type, and their proper motions are exceedingly small—a fact which, together with the high galactic concentration which they show, indicates great distances. There are reasons for believing that they are, on the average, three hundred times as luminous, and $4\frac{1}{2}$ times as far away as the average *A* star of the fifth apparent magnitude. The spectral and photometric characteristics suggest that these stars are super-giants, and so they probably are, though their somewhat high velocities appear to be against the notion that they are exceptionally massive.

It is significant that classes of stars with special characteristics are almost invariably very distant from the Sun; Cepheids, pseudo-Cepheids, many novæ, Wolf-Rayet stars, stars of classes *N*, *R*, and *S*, stars with the *c* characteristic—almost all of these are super-giants, and yet are among the very faint stars: they are very remote, and, excepting classes *R* and *S*, are highly concentrated to the Galaxy. There is an indication here that the Universe is not a simple symmetrical flattened disc without internal structure. In some parts of it, processes take place which are not seen in others. At the present time we happen to be in a region where events occur more or less according to rule, but there are other

places—possibly near the border of the system, where stars may be born—in which the infinite variety of phenomena appears to be more striking than the orderly development from one species to the next. At all events, there is no doubt than no simple scheme of structure or development, fundamentally true though it may be, can account for the diversity of bodies which Nature presents to us in the stellar system.

The more the stars are studied, the clearer it becomes that each of them has its own special characteristics—its own personality, shall we say, which marks it out as an object distinct from all others. Were we to attempt a description even of the outstanding peculiarities of individual stars—any one of which may prove to be the key to the ultimate secrets of Nature—we should require a space far greater than we can afford here. Of the remarkable spectra of β Lyræ and γ Cassiopeiæ and of other *B*-type stars with bright lines ; of the appearance and disappearance of spectrum lines, especially bright ones, without warning ; of the long-period variable, *R* Aquarii, with a typical *Md* spectrum in which, nevertheless, bright nebular lines occur ; of *W* Cephei, whose spectrum resembles that of η Carinæ, and shows bright enhanced lines of iron, like the spectrum of an *S* star ; of the few bright-line stars such as *P* Cygni, which, apart from the Magellanic Clouds, are found only in a very restricted region of the sky ; of the early type " nebulous " stars, which are exceptionally red for their spectral class ; of the many other fascinating problems presented by single stars—we can say nothing. If there is one thing more impressive than the fundamental uniformity of Nature, it is the infinite diversity of her superstructure. She is as careful of the single star as of the type, for, in the last analysis, the single star is the type.

The keynote of our last section was the unity of the stellar system, in which a star was one of a multitude. Its mass, density and temperature circumscribed its individuality; when they were discovered, all was said. The keynote of our present study is the distinctive individuality of every star. The blind, unrelenting forces, laying a basis of uniformity for the Universe, are tempered by the incalculable, half obedient, half defiant influences that make of each star a creation like nothing else in the world. Just as, in human affairs, history unfolds itself by the work of individuals as well as by the advancing spirit of the times, so the destiny of the physical

world, and our knowledge of it, are partly in the hands of single stars. It may be that the discovery of this fact will prove to be the greatest triumph of Astrophysics. We might have expected universal law; we could hardly have looked for universal dissimilarity. It is a great thing to count the numbers of the stars, but it is perhaps a greater to call their names.

CHAPTER XVI

THE NEBULÆ

NOMINALLY, there are two kinds of body in the sky outside the solar system—the stars and the nebulæ : actually there is an indefinite number. Just as the stars show an infinite diversity of character, so do the nebulæ, and with the nebulæ the diversity is the more striking because it is, in part, immediately obvious. The difference between a star and a nebula, broadly speaking, is this : that a star appears as a point, and a nebula as an object of finite size. Of two telescopes, in other respects equal, the one with the larger aperture and greater magnifying power will show the smaller star images and the larger nebula images. The distinction is, perhaps, not perfectly sharp : we have referred to novæ which appear as discs, and there are some nebulæ which appear so small as to be almost indistinguishable from stars. Moreover, some small finite images are doubtless unresolved star clusters. It would, nevertheless, be difficult to name a single criterion which more completely distinguishes the two classes of body.

It follows that a basis of classification is possible for the nebulæ which does not exist for the stars ; namely, the shape of the image. We think of stars—perhaps without sufficient justification—as being invariably spheroidal or ellipsoidal. The Sun and eclipsing binaries provide the only clues we have to stellar shapes, apart from the indications of theory, and they support this idea. But who is to say with confidence what is the shape of a giant *M* or *N* star, which is just at the beginning of its stellar existence ? Possibly the interferometer will be able to settle this question before long : it has already indicated that the diameter of Betelgeuse is varying. In the meantime, the assumption of an ellipsoidal shape is probably the most useful. But the shapes of nebulæ are very diverse. Three types predominate—the diffuse, the planetary, and the spiral—but there are many nebulæ which assume none of these forms. Of the aids to astrophysical research which Nature has provided, there is none for which we should be more grateful than for the possibility of seeing the forms of

nebulæ. Some hidden things may be revealed by indirect means, but what kind of evidence, other than actual seeing, could lead to the discovery that nebulæ have the spiral and other curious forms we now know them to possess? We have a gratuitous starting-point in the investigation of nebulæ which is invaluable.

The classification of nebulæ on the basis of form shows itself to be also a general classification on the basis of space distribution. This has been made clear by Hubble in a recent systematic study of the nebulæ.[1] He points out that diffuse and planetary nebulæ are strongly concentrated towards the Milky Way, while spirals and a number of other forms avoid the Galaxy very conspicuously. He proposes a generalised classification, in which the nebulæ are divided, first of all, into two groups—galactic and non-galactic—and then into sub-groups distinguished by the forms of the images. The detailed classification is as follows :—

	Examples.
I. Galactic nebulæ.	
1. Planetary.	N.G.C. 7662.
2. Diffuse.	
(*a*) Luminous.	N.G.C. 1976.
(*b*) Dark.	Barnard 86.
II. Non-galactic nebulæ.	
1. Spiral.	*M* 101 (N.G.C. 5457, 8).
2. Elongated.	
(*a*) Spindle.	*HV* 24 (N.G.C. 4565).
(*b*) Ovate.	*M* 59 (N.G.C. 4621).
3. Globular.	*M* 87 (N.G.C. 4486).
4. Irregular.	N.G.C. 2366.

Examples of these groups are shown in the plates illustrating this chapter.

A word should be said about the last three divisions of non-galactic nebulæ, which we have not so far mentioned. The spindles may possibly be spirals seen edgewise, but since there is no clear evidence of spiral form, Hubble thinks it advisable to classify them separately. The ovate nebulæ, however, are not spirals at present, if appearances are to be trusted; they are certainly not spirals of the ordinary form. They appear large and bright, and resemble the next class, the globular nebulæ, in everything but form. The globular

[1] Hubble writes that a more detailed classification is now in course of preparation.

PLATE XXXIV.

Orion Nebula.

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nebulæ bear a superficial resemblance to the planetaries, but they have no stellar nucleus, and have a less definite boundary. They are distinguished also by other qualities—velocity, spectrum, etc. The irregular nebulæ, of which only a few are known, are chaotic in form, with a coarse texture. In general appearance they resemble the Magellanic Clouds (see p. 351), more than anything else, though it is not to be inferred that there is any real likeness between the Clouds and these bodies. The whole group of non-galactic nebulæ has a number of common characteristics, which might make it possible ultimately to trace an evolutionary sequence through it.

Considering nebulæ as a whole, however, there seems to be no possibility of forming a sequence such as the Harvard sequence for the stars. Of the two conditions which, in Chapter XII., we saw were necessary to make a theory of evolution possible, the nebulæ satisfy only one: there are many of them, of different kinds, but there is no gradual transition, in general, from one kind to another. Even if it is possible to arrange the non-galactic nebulæ in an evolutionary chain, there remain what appear to be unbridgeable gulfs between these objects, the planetaries, and the diffuse nebulæ. We must recognise at least three totally different kinds of nebulæ, which, if they were re-named to-day, would probably not be given a common name at all. The differences are not only in the forms of the bodies, but in almost every other characteristic as well. The tentative spectral sequence denoted by class *P* includes, it is true, the spectra of both planetary and diffuse nebulæ, but not those of the non-galactic types; and between the former classes, the differences in other characteristics are so marked as to make it practically certain that the spectral resemblance is due mainly to an accidental similarity of physical condition. In this chapter we shall deal separately with three kinds of nebulæ—the diffuse, the planetaries, and the spirals—regarding the last-named as representative of the non-galactic objects, of which they have been studied by far the most extensively.

It is doubtful whether there is any real distinction between dark and luminous diffuse nebulæ. Often the two varieties are seen to pass continuously one into the other, as if, of a single mass, a part were luminous and the remainder obscure. There is, however, a difference arising from this fact—that a

dark nebula is really dark, and not merely too faint for its light to be seen on account of its great distance from us. In this respect, an extended object like a nebula differs sharply from a star. The total light received from a body falls off as the inverse square of the distance of the body from the observer, and that is why some really bright stars are not visible to us : but the brightness per unit of angular area is independent of the distance. For, suppose a nebula at a certain distance gives a certain amount of light per square minute of arc, which we will suppose encloses one square parsec of the surface. If the nebula could be removed to twice the distance, the amount of light received from one square parsec would be only one quarter of the former amount. But one square minute of arc at the larger distance would enclose four square parsecs, so that the loss of light from each unit of area of the nebula would be balanced by the gain resulting from the larger area surveyed. Consequently, a nebula, so long as it is near enough to give an extended image, has the same surface brightness at all distances.

A dark nebula, therefore, would appear just as dark if it were actually very near to us, and a luminous nebula would appear just as faint, as it does now. Since scarcely a nebula is bright enough to be seen with the naked eye, we must conclude that nebulæ, in comparison with stars, are almost inconceivably faint, and visible, even with the largest telescopes, only on account of their enormous size. There might be luminous nebulæ, many times larger than the average star, which are quite unknown to us. It is important to understand what is the source of luminosity in these gigantic bodies, and it will give us a clearer insight into modern ideas of their nature if we consider this problem at the outset.

The light of diffuse nebulæ varies considerably in quality from one nebula to another. The spectra of some of these bodies consist of a few bright lines on a very faint continuous background—the *Pb* type already described, in which the unknown lines, 5007 and 4959, are most prominent. This is often regarded as the characteristic spectrum of a diffuse nebula, but the recent researches of Hubble indicate that a continuous spectrum, generally crossed by absorption lines, is probably the most frequent type of nebular spectrum. There is an unbroken transition among diffuse nebulæ from the continuous spectra into the emission, and it is probable

that the spectrum of every diffuse nebula consists of the two kinds superposed. Faint, continuous light forms the background of even the strongest emission spectrum, and though, when the continuous spectrum is strong, emission lines are not seen, it is not unlikely that they are present in small intensity.

The difficulty in accounting for the source of luminosity in a nebula has always been this. The dimensions of these bodies are inconceivably great. If, therefore, their densities are at all comparable with those of stars, they must contain an enormous mass of matter, and every star in their neighbourhood should be drawn towards them by their gravitational attraction. But no such influence is observed; the stars move apparently as if the nebulæ were absent. Either, then, nebulæ must consist of some form of matter, or primordial stuff, which is not subject to the laws of gravitation, or else they must be exceedingly rarefied, so that the total mass of one of them is not incomparably larger than that of a star. The former alternative is not without plausibility. It fits in very well with the fact that diffuse nebulæ appear to be at rest in space, and also with the presence of unknown lines in the spectra—though, it is true, hydrogen and helium lines appear there also, to say nothing of the continuous spectra. But a hypothesis involving revolutionary assumptions of this kind is always unwelcome, and the alternative idea has found most favour—that the diffuse nebulæ are so rarefied that their masses are not abnormally great. But, with so high a degree of rarefaction as it is necessary to assume, it is difficult to understand how these bodies can be other than intensely cold. There can be nothing, so far as we can see, to check the rapid radiation into space of whatever heat they might possess. And, if they are very cold, how can they continue to radiate light? It would be expected that, in a body without form and void, darkness would cover the face of the deep, instead of which, a large number of nebulæ radiate—faintly, indeed, compared with stars, but still an enormous total quantity of light. The problem is to reconcile the radiation with the rarefaction.

A very ingenious suggestion was put forward a few years ago by Fabry, which, if the postulated conditions actually exist, gives a satisfactory solution of the problem. We know, from Kirchhoff's principle, that a gas, in any given condition, is able to emit only the kinds of radiation which it can absorb.

We know also that, as a body rises in temperature, its radiation, which begins in the infra-red, gradually extends through the visible spectrum into the ultra-violet : the higher the temperature, the shorter the waves emitted. Consider, now, a cold nebula in a universe of stars, and suppose that the material of the nebula is such that the strongest part of its spectrum lies in the far ultra-violet region. The nebula will then be able to absorb light corresponding to this region, which falls on it from the surrounding stars. But it cannot radiate the light away again, because it is very cold and therefore its radiation, if it has any at all, must be radiation of infra-red waves. The absorbed light, then, which cannot get away, must be transformed into heat, and the temperature of the nebula must begin to rise. This will go on until the nebula becomes hot enough to radiate as much energy as it absorbs, and we shall accordingly have a rarefied nebula giving temperature radiation.

It is, of course, essential for the applicability of this suggestion to actual nebulæ that the material in a nebula shall have the properties assumed. We do not know of any substance which has these properties, but it is possible that the unfamiliar green and blue lines in the nebular spectrum arise from an element unknown on the Earth : such an idea has been widely held, and the substance has been named *nebulium*. If nebulium exists, it might behave in the way imagined by Fabry. The tendency nowadays, however, is to regard the varieties of celestial spectra as consequences of a diversity of physical conditions rather than of chemical materials. According to modern ideas, there are very few possible elements left to be discovered, and the probable character of these elements does not favour the likelihood of their association with the simplest elements, hydrogen and helium, in the nebular spectrum.

Assuming, however, that the radiation of a diffuse nebula is an effect of temperature, Fabry and others have measured experimentally what the temperature of the Orion nebula must be, and, incidentally, they have calculated the atomic weight of "nebulium." The principle of the measurement was this. The higher the temperature of a gas, the greater is the average speed of the random motions of its atoms or molecules. If we can measure this average speed, we can calculate the temperature. Now the speed of a radiating atom can be measured by the Doppler displacement of the

spectrum lines it produces. Any line in the spectrum of a nebula, for example, is made up of the radiations from large numbers of atoms, moving in all directions, and the part of the complete line given by each atom will be in a position corresponding to the component of motion of that atom in the line of sight. The edge of the line on the red side will be produced by atoms moving directly away from the spectroscope, and the edge on the violet side will be produced by atoms moving directly towards the spectroscope. The distance between these edges—*i.e.* the width of the line—when converted into an interval of wave-length, will obviously depend only on the speed of the atoms. By measuring the widths of the lines, therefore, the speed of the atoms, and hence the temperature of the radiating body, can be calculated. It is necessary to know the atomic weight of the substance producing the lines, because the temperature corresponding to a given atomic speed depends on the mass of the atom.

Now the Orion nebula shows hydrogen lines, as well as a number of unfamiliar ones (particularly 5007, 4959, and an ultra-violet pair at 3726-29). The atomic weight of hydrogen is known, so that, from the width of the hydrogen lines, the temperature of the nebula can be calculated. Fabry and his collaborators obtained an absolute temperature of 15,000°. Then, having obtained this value, they could apply it, with the measured widths of the unfamiliar lines, to the determination of the atomic weight of "nebulium." They found that the pair at 3726-29 indicated an atomic weight of about 3, while the green and blue lines (5007, 4959) appeared to originate in a substance of lower atomic weight. Apparently, therefore, there were two strange elements in the nebula—a conclusion not inconsistent with the observation that the ultra-violet pair attains its greatest relative strength in a different spectral division (*Pa*), from that characterised by the green and blue lines. The measurements were made with a type of interferometer specially suitable for the purpose, which had previously been devised by Fabry and Perot, and which involved slightly different principles from those of the instrument mentioned in Chapter X.

No substance with an atomic weight of 3 is known. The lightest elements, hydrogen and helium, have atomic weights, respectively, of 1 and 4. If an element of atomic weight 3 exists, therefore, it must be an element of which we have had no experience. But even if we suppose it does exist, we are

no nearer to an understanding of the origin of the unfamiliar spectrum lines. There is the strongest possible evidence that the spectrum of an element depends to an almost negligible extent on the atomic weight: the important thing is the atomic *number*; *i.e.* the number of electrons revolving round the nucleus. Hydrogen has one such electron; helium, two; and the sequence among known elements is unbroken right up to elements of high atomic weight. There is no room for a light element with an unknown atomic number. The evidence of Fabry's measurements, and the existence of unfamiliar lines in the nebular spectra, are therefore not mutually supplementary arguments for the existence of unknown elements in a nebula. Each piece of evidence must fight its own battles, and, to a certain extent, is weakened by the strength of the other. Nebulium, whether single or double, does not easily establish its right to live.

With regard to the measured temperature, however, it is to be observed that, as Fabry himself points out, it is liable to be diminished to a quite indefinite extent if there is considerable turbulent motion in the nebula. In that event, part of the measured atomic speed would be due to such movement, and not to the random motions which determine the temperature. Fabry was able to detect a movement, approximately of rotation, in the nebula, and there were many local irregularities—an observation confirmed later by Campbell and Moore. But, making full allowance for this, the temperature would still remain very high, and, unless Fabry's suggestion of ultra-violet absorption without radiation can be accepted, it is difficult to understand how such a temperature in a nebula can be maintained. Partly on this account, the suggestion has recently been made—or, rather, revived with added force—that the luminosity of a nebula is not an effect of high temperature at all, but a consequence of radiations excited in a cold body by external agencies.

What gives new life to this idea is the discovery that bright stars can be found, either actually involved in or conspicuously associated with almost every known luminous diffuse nebula. The exciting agency can therefore be identified with the radiation (etherial or corpuscular, or possibly both) of the associated stars. The nebulosity surrounding the Pleiades, according to this hypothesis, owes its luminosity to the radiation of the stars in the Pleiades. The great Orion nebula shines under the influence of the familiar " trapezium "

of stars appearing in it. If the hypothesis be true, the apparent outline of a nebula, as Russell points out, bears no relation to its true form. We see the part that is luminous, and there are, in many nebulæ, outlying portions which are too far from the associated stars to be excited. As the star and the nebula drift about in space, the affected region must change; and if our earliest human ancestors could have examined the Orion nebula with high magnifying power, they would probably have seen an object with a totally different aspect from that which we now observe.

The question arises: what causes the differences observed among the spectra of diffuse nebulæ? There are two possibilities—the spectra are determined by the states of the nebulæ, or by those of the stars. If the former explanation were the true one, there would be a basis for establishing an evolutionary sequence among diffuse nebulæ, at least so far as the conditions controlling the spectra are concerned. But it appears that the stars are responsible for the character of the spectrum. Hubble has been able to show that, almost without exception, if the stellar spectra are of type earlier than *B*1, the emission lines are the more conspicuous in the nebular spectra, whereas, if the stellar spectra are of type later than *B*1, the continuous background is the more conspicuous. Nebulæ associated with stars of type *B*1 usually show spectra in which the emission lines and the continuous background have equal prominence. The only important exception to this rule is that a nebula might be inspired to give a relatively strong emission spectrum by an *A*-type star with very pronounced enhanced lines—a characteristic which is usually taken as an indication of the super-giant state. It is a rare occurrence for a Wolf-Rayet star to be associated with a diffuse nebula: the stars which excite emission spectra in the nebulæ are almost wholly confined to the small range, *Oe*5—*B*1, while those which excite continuous spectra are very infrequently of type later than *B*8. It is evident that the conditions of the *B* stage are necessary in a star to make a neighbouring diffuse nebula visible.

The widespread association of bright stars with luminous diffuse nebulæ, and the definite relation between the spectra of the stars and those of the nebulæ, are, in themselves, almost a convincing argument for the substantial truth of the hypothesis we are considering. Hubble, however, has applied a further test, the result of which gives the hypothesis yet

more support. Assuming that the effective quality of the stellar radiation is proportional to the visible intensity, there should be a relation between the area of luminous nebulosity and the absolute magnitude of the star, for, the brighter the star, the greater will be the extreme distance at which its radiation will operate. Since the star and nebula must be at approximately the same distance from us, the same relation should exist also between the *angular* area of the nebula and the *apparent* magnitude of the star—though, it is true, the relation might be obscured owing to the unknown amount by which the light of the star is absorbed by the nebula. Hubble finds that the expected relation is followed very closely, though the deviations shown by some of the systems indicate that the light of the corresponding stars is partly absorbed—an interpretation supported by the relation of the colour indices of the stars to their spectral types (p. 121). It is clear, however, that the evidence for the excitation theory of nebular luminosity is very strong. It may be remarked here that it removes one strong objection to which Russell's theory of evolution was originally open—that the association of diffuse nebulæ with *B* stars, and not with giant *M* stars, implied that the former were in the earliest stages of their stellar life. It appears now that the association is between the *luminosity* of the nebulæ and the *B* stars, and gives no indication at all of a *material* relation between the two types of object.

It will naturally be asked: how do the stars cause the nebulæ to shine? The question is not at all an easy one. It may be that the nebulæ simply reflect the light of the stars, or, on the other hand, they may absorb the light and re-emit it by the process which we call *fluorescence* when we find it occurring on the Earth. The emission spectra, at least, cannot be due to reflection, because they are quite different from the spectra of the associated stars. We must suppose a kind of fluorescence here, though whether it is of the same kind as terrestrial fluorescence may well be doubted. The latter process depends to a large extent on the wave-length of the exciting radiations, whereas, in the nebulæ, it is, apparently at least, the intensity of the radiations that counts. The continuous spectra are, in many instances, identical with the spectra of the involved stars—good slit spectrograms show that they are crossed by absorption lines, which reveal the spectral type: this occurs, for example, in the Pleiades, and

the well-known nebulosity in the region of the star ρ Ophiuchi. This points to reflection of the stellar light, but Hubble has given reasons for supposing that what takes place is really an absorption, and re-emission without change, of the light of the star. The appearance of the resulting spectrum, if that were so, would be the same as that of the spectrum produced by direct reflection, but the physical process occurring in the nebula would be different. It would be essentially of the same kind as that responsible for the line spectrum, but the nebular light, instead of being concentrated in a few wave-lengths, would be spread out through the spectrum range. The question is of some importance, because of its bearing on the material constitution of diffuse nebulæ.

This also is a difficult question. It appears that, on the whole, the nebulæ are opaque, though there are a few instances in which stars can be seen through them. It is probable, therefore, in view of their extreme tenuity, that they consist, at least in part, of fine dust—the most effective kind of light-screen. On the other hand, the emission of a line spectrum suggests a gaseous origin. Russell pictures a nebula as a cloud of dust in a mass of gas, the tenuity of the whole being maintained by the equilibrium of stellar radiation pressure and inherent gravitational consolidation. He regards the continuous spectrum as the result of reflection of the stellar light, presumably by the dust particles, and the line spectrum as the contribution of the gaseous portion. It is somewhat difficult, on this view, to understand why the proportion of light reflected should vary so widely as it does from one nebula to another. The radiation of a *B*o and a *B*8 star must be mostly identical in kind, yet we find scarcely any of the light of the one reflected, and scarcely any of the light of the other concentrated in a new emission spectrum. This is one of the reasons why Hubble is inclined to reject the idea that a nebula reflects light appreciably. But it is not clear that the difficulty is removed if we substitute re-emission for reflection. It has still to be explained why the re-emitted light varies so greatly in quality for so small a change in the light of the star.

Spectroscopic examination of two of the most prominent diffuse nebulæ—the Orion nebula and the Trifid nebula in Sagittarius—shows that the "nebulium" lines are relatively strong in the central bright parts of the nebulæ, while in the outer parts, hydrogen lines predominate. From our present

point of view, this gives no indication of the distribution of the material in a nebula, but shows us, rather, how the same material behaves under the influence of radiation of varying intensity. It is probable that the exciting radiations are active only at the surface of the nebula: what we see is simply the silver lining to the vast obscure cloud behind. In this respect, though for a different reason, the nebulæ disappoint us just as much as do the stars: they reveal to the spectroscope only their superficial aspect, leaving us to guess at the character of their interiors. It can scarcely be doubted, however, that the interior of a nebula is essentially similar to the surface, though what that might be is still largely matter for speculation.

Turning now to the other characteristics of diffuse nebulæ —characteristics such as those dealt with in Section II. with respect to the stars—we note first one or two points connected with magnitude. The magnitude of a nebula is not a matter of very great interest: what is important is the surface brightness, and this we have seen to be in all cases very small compared with that of the stars. Two special phenomena call for a little attention—the dark nebulæ and the variable nebulæ.

The recognition of dark nebulæ—or, at least, the realisation that they exist in considerable number—is due chiefly to Barnard. These bodies can be detected by the existence among the stars of apparent vacancies, like empty spaces, of irregular shape, against which few stars appear. It is probable that there are such empty spaces, but the majority of the appearances are now definitely believed to be due to dark nebulæ interposed between the Earth and more distant stars. There is no infallible rule for distinguishing between a nebula and a vacancy: usually it is a matter of assigning the most probable interpretation to the appearance. Plates I and XXXV, for example, it can hardly be questioned, represent actual dark bodies. Barnard has pointed out that a dark nebula appears, in a photograph, to be darker than the surrounding part of the sky in the spaces between the stars. He suggests that the sky is filled with a faint luminosity, due to numberless distant stars which cannot be seen as individuals; the dark nebulæ, by obscuring this light, appear darker than their background. Father Hagen maintains that dark nebulæ can best be detected by direct visual observation; he believes that they are far more numerous and widely

PLATE XXXV.

Nebula *Barnard* 92.
(Galactic—Dark Diffuse).

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distributed than they have generally been supposed to be.

Variable nebulæ appear to form a small class by themselves. They are generally in the form of a fan, with a variable star at the radiant point. A well-known example is Hind's variable nebula in Taurus. It was once a conspicuous object in a small telescope, but about sixty years ago it suddenly disappeared, and became invisible even when looked for with the largest telescopes. It can now just be seen with the most powerful instruments. It is associated with the variable star, *T* Tauri—a star with an *Md* spectrum containing more bright lines than are usually found in the spectra of such stars. The light changes of *T* Tauri have not been comparable with those of the nebula—a fact which seems to indicate that it is not the intensity of the visible light of the star which is mainly responsible for the luminosity of the nebula. Brown has pointed out that the fan-like shape of these bodies can be explained by supposing that the star has rushed through the nebula, provided the nebula can be conceived as a swarm of small particles. This type of catastrophe we have already suggested as the explanation of novæ. But we know so little about the probable circumstances of such an event that we cannot deny the possibility that either phenomenon might arise from the same cause, according to the conditions of the encounter and the character of the bodies concerned.

The velocities of diffuse nebulæ are small compared with those of the stars. Campbell finds an average radial velocity of about seven miles per second for five of the bodies having bright-line spectra. The radial velocity varies, however, over the area of a nebula. In the Orion nebula, for instance, there is an extreme variation, from one part to another, of nearly ten miles per second : the nebula as a whole appears to be at rest in the stellar system. Curtis, from scanty data, has given a rough estimate for the average annual proper motion of diffuse nebulæ, of 0″.036. But if the luminosity of a nebula arises from a neighbouring star, the proper motion observed is merely the motion of the luminosity, and not that of the material with which it is temporarily associated. The proper motion, in that event, would measure a property of the star rather than a property of the nebula. On the whole, it appears that the motions of diffuse nebulæ are mostly of a turbulent character : there is no clear evidence

of the regular motion of the centres of gravity of these objects.

The same difficulties that face the determination of the proper motions of diffuse nebulæ are presented also to the measurement of their parallaxes. The associated stars, if they are known, may be used for the purpose, but the presence of the nebulæ, and the consequent confusion of the stellar images, usually make direct measures impossible. A few parallaxes have been determined in this way, however, *e.g.* the parallax of the Orion nebula has been found to be about 0″.005. But, in the main, indirect means have to be adopted. One of the most interesting determinations of this kind was made by Andersen, in an investigation of the parallax of the " North America " nebula. This nebula is an opaque body, and the stars apparently superposed on it must therefore be at least as near to us as the nebula itself. Now, as we know, there is a statistical relation between the apparent magnitudes and the distances of stars: stars of the first magnitude are at a smaller mean distance than stars of the second, and so on. Andersen counted the number of stars of different magnitudes, both in and just outside the line of sight of the nebula. For the brighter stars, the numbers were approximately the same in both directions, because these stars were all nearer than the nebula. At a certain magnitude, however, the relative number of stars in the line of sight of the nebula began decidedly to diminish, because, in the surrounding space, some stars of this magnitude were included which were more distant than the nebula. By observing the magnitude at which this occurred, and using Kapteyn's formula connecting the parallaxes of stars with their apparent magnitudes (p. 112), Andersen estimated the parallax of the nebula to be 0″.007. It is probable that the parallaxes of diffuse nebulæ in general cover a considerable range. From proper motion data, Kapteyn finds a mean parallax of 0″.005 but, as we have seen, the apparent proper motions of these bodies are possibly not real.

The temperatures of diffuse nebulæ have already been referred to: it is probable that they are very low. The densities also must be supposed to be low. The total mass of a diffuse nebula is very difficult to estimate, because of the lack of data certainly appropriate to its determination. Russell, on certain assumptions, has suggested the order of magnitude of the mass of the Orion nebula. He supposes that

the material of a nebula tends to be dissipated by radiation pressure from the bright giant stars, and that the dissipation is opposed by the gravitational cohesion of the nebula. Using the observed extreme departure from the mean velocity in the Orion nebula, of 5 kilometres per second, he has calculated what the gravitational attraction should be in order that the radiation pressure should maintain this velocity. He finds that the nebula should contain enough matter to form hundreds of stars, and that a sensible fraction of the whole must be dust, composed of particles whose diameters are less than one-tenth of a millimetre. There is no prospect at present of a more definite determination.

The distribution in space of diffuse nebulæ is a matter which has recently called for re-consideration. The luminous diffuse nebulæ are almost entirely confined to the galactic plane, and it has always been supposed that this is one of their fundamental characteristics. But if the nebulæ are luminous only because of the presence in their midst of bright stars, then the supposed galactic concentration of diffuse nebulæ may be merely a consequence of the known galactic concentration of the early type stars. The really significant thing would be the distribution of diffuse nebulæ, irrespective of whether they are bright or dark. And here again a spurious appearance of galactic concentration might arise, because dark nebulæ also stand a better chance of discovery where the bright objects are thickest and the background is consequently more nearly continuously bright. This must be allowed for in considering the actual distribution of the nebulæ in space.

Hubble, after a close photographic investigation, concludes that the dark nebulæ actually do show a high galactic concentration. They cluster in the plane of the Milky Way, and also in a plane inclined to it at a small angle, the significance of which we shall be able to appreciate better in the next section. He finds no diffuse nebulæ at all with a galactic latitude higher than 37°, and the single one having this latitude is quite exceptional with regard to its position. Hagen, however, using visual methods of detection, claims that the dark nebulæ are densest at the galactic poles. He regards the whole galactic system as a hollow in a mass of dark nebulous matter. The boundaries are much nearer in the directions of the galactic poles, so that the dark clouds there—presumably made faintly visible by the light of the

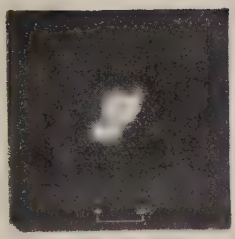
neighbouring stars—are more conspicuous than are the more distant ones outside the Milky Way. An absolutely clear sky is found only in the immediate neighbourhood of the densest star clusters, and the apparent absence of nebulæ from these regions is probably an optical effect, due to their extreme distances. Hagen thinks that the bulk of the dark nebulous matter does not form absorbing clouds at all, and that the isolated fragments which show a galactic concentration form only an inconsiderable fraction of the whole.

Hagen at present stands almost alone in support of these views. Visual observation is, at best, a somewhat risky path to a knowledge of very faint objects, and there are several byways which might easily be mistaken for the direct road. The general belief at present is that the diffuse nebulæ—dark and bright—are interspersed among the stars, and are highly condensed to the galactic plane or its neighbourhood. What their relation to the Galaxy may be, and whether they are the parents or the offspring of stars, are questions which we shall take up later, but to which it is not yet possible to give a definitive answer. We must proceed now to a consideration of the planetary nebulæ, which occupy the remaining division of Hubble's classification of the galactic nebulæ.

Planetary nebulæ differ from diffuse nebulæ most obviously in form : there is a regularity of some kind in their outline that is absent from the diffuse types. As a rule, these bodies appear in photographs as round discs, like planets—hence their name, which they owe to Sir William Herschel. Planetary nebulæ, however, now include a number of less simple forms. Sometimes the nebulosity appears in a circular or elliptical ring, as in the well-known example in Lyra. In other planetaries, there are " wings," or points of apparent condensation, situated symmetrically with respect to the centre. There is a considerable variety of form among these bodies, but symmetry of some kind is their distinguishing characteristic. They are of very rare occurrence—less than 150 are known, in spite of a close survey of the sky.

The variety of form, though it remains very noticeable, is found, on closer examination, not to be so great as it appears. It is doubtful if there is any real difference between the disc and the ring-shaped nebulæ. Photographs in monochromatic light show that the discs are made up of a number of concentric rings, which fit into one another and so appear as a

PLATE XXXVI.



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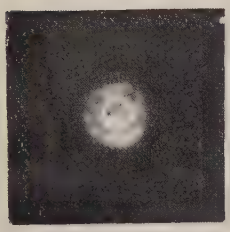

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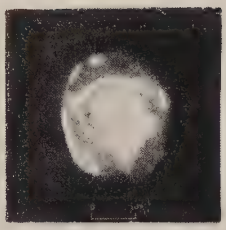

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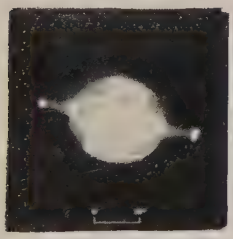

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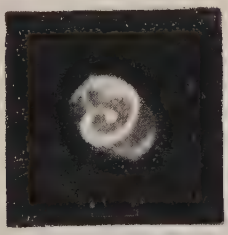

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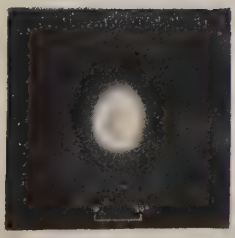

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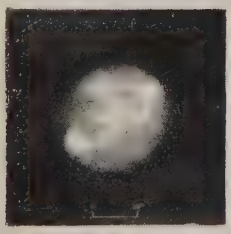

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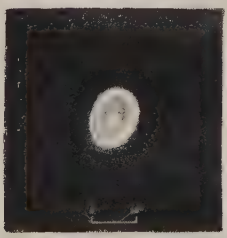

I.C. 418

Varieties of Planetary Nebulæ.
(Galactic)

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continuous surface. Each ring emits chiefly light of a particular wave-length, though the light of the outer rings may be emitted more feebly by the central parts of the disc also. We may regard the whole disc as made up of adjoining rings, or—perhaps more correctly, in the majority of nebulæ—as a number of superposed discs, of differing radius. The character of the light emitted by the various discs is generally as follows. The smallest disc, around the centre, is responsible for the unknown line at λ3426 : λ4686 (ionised helium) is emitted by the next disc : then, as a rule, appear the green and blue " nebulium " lines, λλ5007 and 4959 : next, the unknown line at λ3869 ; and finally the unknown pair at λλ3726-29. A planetary nebula is evidently not a homogeneous structure.

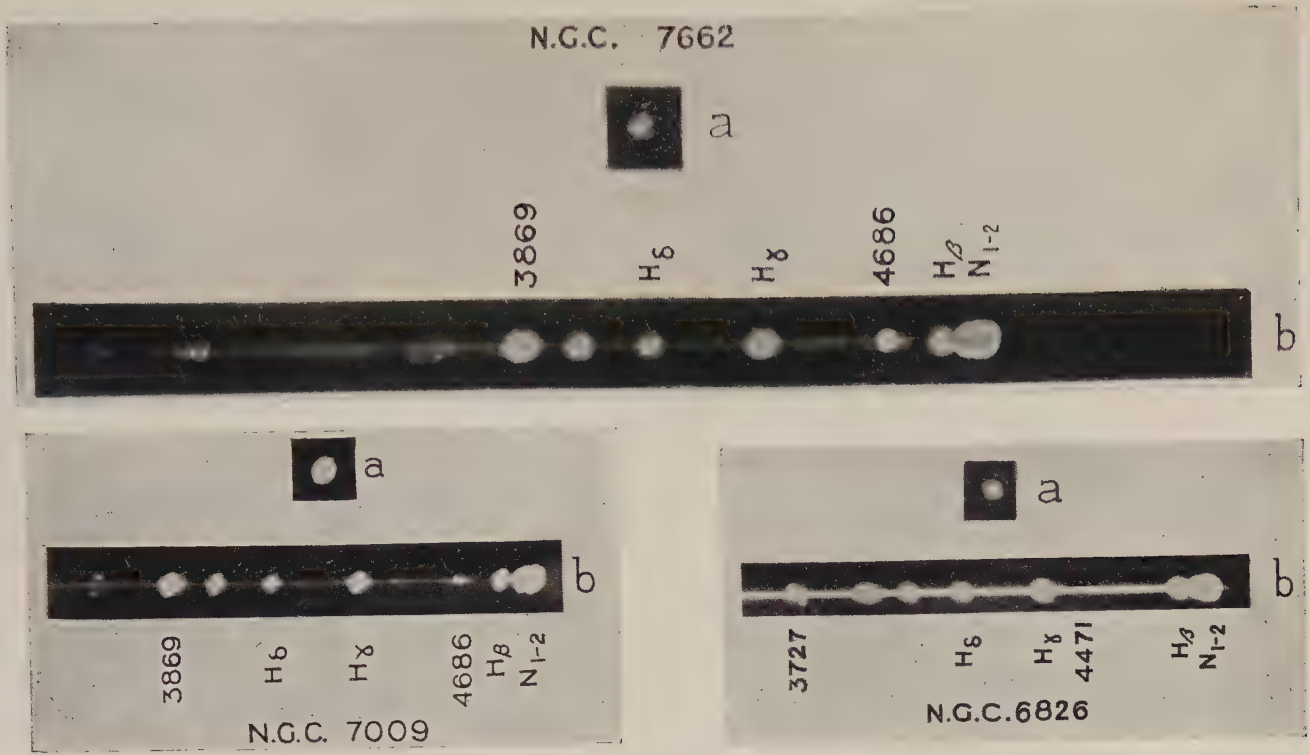
The analysis of the nebula along these lines is carried out by means of slitless spectrograms. When the slit of the spectroscope is removed, each " line " in the spectrum becomes an image of the source of light instead of an image of the slit. Thus, each image is a photograph of the region of the source which emits the light in it. Plate XXXVII shows a few slitless spectrograms of planetary nebulæ, obtained by Wright at the Lick Observatory : the difference in size of the images will be noticed. It should be remarked, however, that the analysis of the nebula into discs or rings is not absolute : it marks the regions in which the light of particular wave-lengths predominates but is not totally confined. Long exposures, in which faint light can make as deep an impression on the plate as the stronger light, would hide the effect by making all the images tend to become uniform in structure and equal in size. The conditions of photography have to be adjusted by trial in order that the most instructive results shall be obtained.

In speaking of " discs " or " rings," we refer, of course, only to the appearance presented to the eye or the photographic plate : it must not be concluded that the nebulæ are actually flat structures. It is probable that the majority, at least, consist of spherical or ellipsoidal *shells*, which are seen in projection on the celestial sphere. The evidence for this conclusion is that none of them even approximately presents its edge to us, as a number almost certainly would do if they were plane bodies. The ring-like appearance of some of them is due to the fact that the thickness of the material of a shell in the line of sight is greatest round the

edges. It should be mentioned, however, that the relative intensities of the light from the edges and the centre in some of the nebulæ are difficult to reconcile with this idea. Curtis has suggested slight modifications of the simple shell structure which may possibly explain the anomalies.

The outer rings or discs are usually more elliptical than the central ones : there seems to be a gradual increase in ellipticity from the centre outwards. The outermost ring, composed of the wave-lengths 3726-29, usually shows much more detailed structure than the others. The ends of the major axis of the ellipse are often abnormally faint, and there are, as a rule, many irregularities in the density of the light throughout the image. This may be due to a non-uniform distribution of the material of the nebula, or, equally well, to a greater sensitiveness of the wave-lengths concerned to variations of physical condition in their source. Before we can decide between these alternatives, we must know more about the constitution and conditions of luminosity of planetary nebulæ than we do now.

The nature of a planetary nebula is just as difficult to determine as is that of a diffuse nebula. The two kinds of body, however, have one thing in common—they are almost invariably associated with stars. In the planetary nebulæ, this association is more conspicuous than it is in the other class, because of the symmetry of the bodies : the star appears as a rule, at the centre of the disc or ring, and, even by its position alone, suggests to us that it is connected with the nebula. The rule, however, is not without exception. In one nebula, for example—the double-ring nebula in Gemini (N.G.C. 2392)—the star is eccentric, and there are some planetaries which show no star at all. With regard to the latter, it should be observed that there is almost invariably some special characteristic connected with nebulæ which appear to have no associated star. They are perhaps apparently very small, so that the star may be there, but invisible ; or they show a central condensation which may perform the same function—whatever that might be—as the star in the majority of the planetaries ; or they exhibit some other peculiarity. Curtis, after an exhaustive study of these bodies, concludes that " there will be but slight chance for error if the definite rule be laid down that a typical planetary nebula has a central star." We shall therefore assume, in



Slitless Spectra of Planetary Nebulæ.
(*a*) Direct Photograph. (*b*) Prismatic Photograph.

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our discussion of planetary nebulæ, that a central star or its equivalent is an essential part of their existence.

In view of the relation between diffuse nebulæ and stars, we can hardly avoid the idea that the central star in a planetary nebula is the source of luminosity of the nebula itself. The probability of this idea is strengthened by the fact that the spectra of planetary nebulæ show the same unknown lines, and are, in general, of the same type, as those of the diffuse nebulæ: according to Wright, the spectra of the planetary nebulæ differ much more among themselves than some of them do from the spectrum of the Orion nebula. Why is it, then, that there is such a pronounced difference in form—and, as we shall see presently, in a number of other characteristics also—between the two types of nebula? This question has, up to the present, received no final answer. It may be, however, that the type of the central star has something to do with it. In a diffuse nebula, the spectral type of the associated star makes all the difference in the matter of luminosity or obscurity, and also in determining the spectrum if the nebula is made to radiate. A study of the nuclei of planetary nebulæ is therefore suggested.

As we have before pointed out, the nuclei of planetary nebulæ are Wolf-Rayet stars. This fact has been established as the result of a comprehensive investigation by Wright. The evidence for the conclusion is not of the same kind in every instance. In only a few of the nuclear spectra do the characteristic Wolf-Rayet broad lines predominate. In a large number they appear on a continuous background which forms the most noticeable feature of the spectra, and in about half of the spectra the continuous background is all that can be seen. Occasionally, dark lines are noticed also. So far as the line spectrum is concerned, therefore, a large number of the nuclei are not typical Wolf-Rayet stars. But when we consider the distribution of energy in the continuous spectrum, we are forced to conclude that here the nuclei are undoubtedly of the Wolf-Rayet type. The amount of energy grows at an ever-increasing rate as we go from the red towards the violet: it has its highest value at the point where the spectrum is cut off by general absorption in the Earth's atmosphere, and is there increasing at a rate which, when interpreted by Planck's theoretical formula for the variation of energy-distribution with temperature, indicates an effective temperature of the order of 50,000°! We pointed out in the last

chapter that, for Wolf-Rayet stars, the relation between the continuous spectrum and the temperature might not be strictly comparable with the corresponding relation for the stars of other types : a temperature of this order must therefore not be accepted as the necessary interpretation of the observations. But the point which is important for our immediate purpose is that the same phenomenon is shown by Wolf-Rayet stars which have no associated nebulæ. Combining, then, the facts that half of the nebular nuclei show Wolf-Rayet lines, and all of them show a continuous spectrum (which may possibly, in the other half, mask the line radiations) with the same type of energy-distribution as that of the continuous spectrum of Wolf-Rayet stars, there are very strong grounds for the conclusion that the nuclei of planetary nebulæ are invariably Wolf-Rayet stars. This is a very important conclusion, though what its full implications are is not yet known.

If we accept the hypothesis that the luminosity of a nebula, whether diffuse or planetary, is due to radiations from the associated star, it is hard to avoid the conclusion that, for some reason, a Wolf-Rayet star is able to stimulate a symmetrical envelope, while an *Oe*5 or *B* star acts in a much more irregular way. If a star is situated in the midst of an extensive, fairly homogeneous nebula, and radiates equally in all directions, then the nebula must appear in a planetary form. Apparently, Wolf-Rayet stars alone fulfil these conditions. We must probably suppose, therefore, that the relation between the stellar and the nebular parts of the systems is more intimate in the planetaries than it is in the diffuse nebulæ. If, as we were led to consider in the last chapter, planetaries are associated with novæ, then the various parts of their nebular envelopes will depend for their properties on their distances from the stars, and the forms of the bodies will naturally be symmetrical : we should anticipate, in fact, from our experience of novæ, that the structures would be formed of concentric shells. The Wolf-Rayet character of the stars would be due to their history—that is, ultimately to their association with the nebulæ—and, instead of having to explain why a Wolf-Rayet star stimulates a planetary nebula, we should have to explain why a planetary nebula—regarded as the metamorphosis of a nova—should leave the original star with Wolf-Rayet characteristics. The diffuse nebulæ, on this view, would have to be regarded as

independent bodies in space, which happen to be in the neighbourhood of bright stars. The stars might be of any type, but only *Oe*5 and *B* stars, generally speaking, would be able to make the nebulæ luminous. These stars could never become "novæ," *i.e.* they could never rush into the dark nebulæ, because, owing to the pressure of their radiation, the nebulous matter would retreat in front of them and keep them at a safe distance. They could therefore never stimulate a nebula symmetrically from the interior, because they could never get into the interior. This supposition appears to be consistent with the facts, and opens the way to interesting lines of inquiry.

The interpretation of planetary nebulæ as evolved novæ raises the question: why are some Wolf-Rayet stars associated with nebulæ, while others apparently are not? Until we know much more about the conditions in a Wolf-Rayet star, this question will probably remain unanswered. It may be that there are actually two classes of Wolf-Rayet star —those which once were novæ and now form the nuclei of planetary nebulæ, and others which possibly are ultra-*B* stars and are unassociated with nebulosity. In support of this idea is the fact that the exceptionally faint Wolf-Rayet stars are nebular nuclei. But none of the Wolf-Rayet stars which have been followed from the nova condition show typical nebular atmospheres; their nebular spectra abdicate at a comparatively early stage in favour of the Wolf-Rayet spectra: and, further, no existing planetary nebula has ever been observed as a nova. If planetaries are developed novæ, they must have taken at least hundreds of years to reach their present state—which, it is true, is not a very startling hypothesis. On the other hand, it is possible that both kinds of Wolf-Rayet star are the same in origin, but are at different stages of evolution. This possibility has to face more difficulties than the former one, but since our ignorance on the matter is all but absolute, it cannot be left out of account.

Wolf-Rayet stars altogether are essentially mysterious bodies: in some ways they are the negation of the best-established conclusions of Astrophysics. What, for instance, are we to make of the rapid increase of energy as we move towards the ultra-violet regions of their continuous spectra? The light from the nucleus of a planetary nebula has to reach us through a vast envelope of nebulous matter, and the

probabilities are all in favour of a large selective scattering of ultra-violet light. Moreover, if the light stimulates luminosity in the nebula, it is ultra-violet light again that would almost certainly be in greatest demand for the purpose. If we make any apparently reasonable allowance for these operations, the energy curve becomes fantastic. Experience and theory alike fail us when we attempt to interpret the hardly questionable observations.

We are forced for the present to leave these problems unsolved. It may well be, however, that the excess of ultra-violet light in the radiations from the nuclei of planetaries denotes radiation mainly from the surface, and thereby supports the idea that the stars originated as novæ : though, since the enormous nebular atmosphere fails to redden the star, we are, perhaps, hardly justified in concluding that passage of the light through the star itself would necessarily do so. Again, it may be that, in the few planetary nebulæ which have no apparent nuclei, there are stars whose most copious radiations lie yet farther in the ultra-violet than those of the majority, and that the stars are not found because nearly the whole of their radiation is absorbed by our own atmosphere. This, in itself, appears very improbable, but not more so than the spectra of the observed nuclei, which we know to be real. When facts go beyond the bounds of probability, speculations are entitled to a similar freedom.

In their general characteristics, planetary nebulæ show no relation to the Harvard sequence. Their absolute brightness is small : van Maanen finds an average absolute magnitude of 9.1 for six of the bodies, whose individual absolute magnitudes are very close to the mean value. The observations were confined to the nuclei, which emit by far the greater amount of the total light. Since Wolf-Rayet stars which have no associated nebulosity have absolute magnitudes about zero, van Maanen's result gives considerable support to the hypothesis that Wolf-Rayet stars belong to two distinct classes. The very meagre data for the absolute magnitudes of novæ at their minima indicate a value of about 7, so that, so far as magnitude is concerned, planetary nebulæ might very well have developed from past novæ.

With respect to velocity, there is again a distinction between planetary nebulæ and Wolf-Rayet stars without nebulosity. The latter have low velocities ; the planetary nebulæ have very high ones. Curtis estimates the proper

motions, from a small amount of data, to be of the order of 0″.028 per annum, but the most definite evidence comes from radial velocity determinations by Campbell and Moore. These investigators find that the average radial velocity of 96 planetary nebulæ is about 19 miles per second—five times as large as the average radial velocity of the *B* stars. In addition, there are six planetary nebulæ, appearing in the same region of the sky (the Scorpio-Sagittarius region again), which have exceptionally high radial velocities—greater than 72 miles per second. It is clear that planetary nebulæ cannot at present be placed next to the *B* stars in the evolutionary sequence. Velocity measurements, as well as other considerations, place them outside the normal course of events.

In their studies of radial velocity, Campbell and Moore obtained a considerable amount of evidence of internal motion in planetary nebulæ. Of forty-six of the bodies, twenty-five showed motion of this kind, and, in the majority of these (the apparently largest and intrinsically brightest), the motion could be explained as a rotation about an axis approximately perpendicular to the line of sight. In four of the nebulæ, however, the motions were not so explicable. It appears that the most elongated nebulæ show the highest rotational speeds—a fact which can be explained by supposing that the elongation denotes ellipticity brought about by rapidity of rotation. The motion is not that of a solid body, however; the outer layers lag behind the inner ones. The exact interpretation of the observations is very difficult in some instances, because of a curious distortion and doubling of the spectrum lines. No indubitable explanation of this phenomenon has been given, but it must be remembered that, since the planetary nebulæ are very transparent, the light coming in any direction from a nebula comes from the whole thickness of the nebula in that direction, and will therefore possibly be produced under a large variety of conditions.

The parallaxes of planetary nebulæ probably cover a very wide range. The apparent diameters, no doubt, give some indication of the relative distances, and these vary from the so-called "giant" planetaries—an unfortunate term, in view of its quite different meaning when applied to the stars—to the "stellar" planetaries—bodies so small as to be only just distinguishable from stars. Van Maanen has measured the parallaxes of six planetary nebulæ by applying

the trigonometrical method to the central stars. He finds an average parallax for these bodies of 0″.018, with individual values ranging from 0″.008 for N.G.C. 6720 to 0″.023 for N.G.C. 7662. The planetaries chosen by van Maanen were all apparently larger than the mean.

The temperatures of planetary nebulæ are quite unknown. We have already referred to the difficulty of estimating the temperatures of the central stars, and we have no means at present of estimating those of the nebulous atmospheres. With regard to colour, Lundmark finds an effective wavelength of just under λ4160.

Assuming the observed rotation of a planetary nebula to be an actual rotation of the whole mass according to ordinary dynamical laws, Campbell and Moore have calculated the probable masses of three of the bodies for which parallaxes have been determined. They find N.G.C. 7009 to be 162 times as massive as the Sun, and the other two to be between 4 and 20 times as massive as the Sun. The estimates are subject to some uncertainty because of uncertainties in the parallaxes, and the neglect of influences such as light pressure which must modify the action of dynamical forces. There is a general consensus of evidence, however, derived from about 20 planetaries, that these bodies are more massive than the solar system. Their diameters can be found only when the parallaxes are known. Van Maanen found, for the six whose parallaxes he measured, diameters varying from just below 0.01 parsecs to 0.05 parsecs—a range of from 1350 to 10,000 astronomical units.[1] These dimensions are enormous, and if the nebulæ are actually made luminous by the central stars, it must follow that stars whose absolute brightness is apparently much less than that of the Sun can excite luminosity over distances approximately fifty times the radius of Neptune's orbit. This is a very remarkable conclusion, and is at first somewhat difficult to credit: but we must remember that the nebulæ are remarkably transparent to ultra-violet light, and, further, that Rigel (β Orionis)—a star far brighter than planetary nuclei, but one emitting a far smaller proportion of ultra-violet radiation—gives evidence of stimulating surrounding nebulous matter over a distance of more than six parsecs—greater than the distance of the Sun from Sirius.

[1] An *astronomical unit* is the mean distance between the Sun and the Earth.

(*a*)

(*b*)

(*c*)

(*d*)

Spiral Nebulæ (Non-Galactic).

(*a*) Canis Venaticorum 7. (*b*) M. 64 Comæ Beren.

(*c*) Great Andromeda Nebula. (*d*) Nebula H.V. 24 Comæ Beren.

To face

Another remarkable fact concerning the diameters of planetary nebulæ has recently been pointed out by Hubble: as the apparent diameter increases, the type of the central star advances. Thus, while the central stars of the stellar planetaries are of the true Wolf-Rayet type, those of the "giants" are of type *Oe*5. If the diameters are all essentially equal, this must be a correlation between the spectral type of the nucleus and distance from the Sun. It is hard to suppose that a correlation of this kind can exist: it is much more likely that the nuclei are related to the actual diameters, and if so, it seems that the later type stars are effective throughout a larger sphere than are those of earlier type. This may be an effect of absolute magnitude, but it is evident that we must await a fuller knowledge of the physical processes allied to fluorescence before the problems presented by planetary nebulæ can be solved satisfactorily.

Planetary nebulæ, like novæ and Wolf-Rayet stars, are almost entirely confined to the galactic plane. There is evidence that they exist in groups: we have already mentioned that six of them, with exceptionally high velocities, are found in the same part of space. Campbell and Moore point out also that the "stellar" nebulæ appear to form a system concentrated in one quadrant of the Milky Way centred at Right Ascension, 19 hours: they are probably farther away from us than the apparently larger ones. Exceptional in origin and character, planetary nebulæ appear to be exceptional also in distribution. They have contributed little to the solution of the more general astrophysical problems, but the interest they excite is out of all proportion to their numbers.

The charge of scarcity can certainly not be laid at the door of the non-galactic nebulæ. These bodies exist in remarkable profusion, and must be recognised as normal products of the processes of Nature. There is reason to believe that at least a million are within reach of modern telescopes. They abound in all directions in space except in the galactic plane, where not a single one has been found. This absolute avoidance of the Galaxy is their most striking characteristic, in which they are quite unique: the plane in which all other celestial bodies are most thickly crowded is the one region which they avoid. Whatever theory may be held with regard to their function in the Universe will remain conspicuously

incomplete so long as it does not offer an explanation of this remarkable distribution.

As we indicated on a former page, we shall consider spiral nebulæ as representative of the non-galactic types—not because their properties are certainly identical with those of the other kinds, but because they have been studied by far the most extensively. Indeed, there is not, as yet, a general admission that nebulæ other than spirals exist in appreciable numbers outside the Galaxy, though it is exceedingly probable that they do. If so, it will be one of the problems of the future to examine their qualities and assign to them a place in the general scheme of things. For the present, our knowledge is practically confined to the spirals.

Allowing for the fact that we see spiral nebulæ at all angles, it appears that they have not a great range of variety of form. The general description, of a central flattened nucleus with two spiral arms issuing from opposite ends of a diameter, is probably true of all of them. The arms are irregular, being constructed apparently of nebulous material with bright condensations at intervals along their course. In a large number of those which we see edgewise, or nearly so, dark occulting matter appears, sometimes outside the arms and sometimes within the whorls (see Plate XLII.). The nebulæ have a very large range of apparent size, from the great nebula in Andromeda down to objects so small as to be hardly distinguishable from stars.

In discussing diffuse and planetary nebulæ, we began with a consideration of their nature and character, and then proceeded to an account of their condition with respect to the qualities common to all bodies. Such a treatment is impossible for the spirals because, apart from the common qualities, we have no grounds on which to form any idea at all of their nature. It is generally agreed that they shine by their own light,[1] and probably it is assumed by every one that high temperature is mainly responsible for their radiation. But their physical constitution, their place and purpose in the Universe, and their relation to the other celestial bodies, are matters with regard to which we can only make more or less probable deductions from observations of the ordinary kinds. It is necessary, therefore, that we should consider those observations at the outset.

[1] Lindemann, however, has just suggested that they reflect the general star-light of the Galaxy.

The spectra of spiral nebulæ have been investigated by Fath. He finds that the nuclei show, as a rule, a spectrum of *F* or *G* type. There is, however, a fair amount of variety. Fath finds an almost continuous sequence from spectra consisting chiefly of bright lines (including the characteristic nebular radiations at λλ5007, 4959), to absorption spectra of *K* type. It does not appear that the spectra of the arms have been studied, but Seares has shown that, in spirals whose nuclei show a spectrum approximately of solar type, the arms are decidedly bluer than the nuclei. This would point to an earlier type of spectrum for the arms. There is nothing in the spectra of spiral nebulæ, so far as we know, of a novel character. In this quality, at least, we can apply to the spirals the principles used in the discussion of the bodies we have already studied.

The apparent brightness of spirals is very small: it was not until Lord Rosse constructed his great reflecting telescope about the middle of the last century that their true form became known. The nuclei appear much brighter than the arms. The nucleus of the great Andromeda nebula can be seen almost with the naked eye, but prolonged exposure to a photographic plate is necessary to reveal the spiral structure. The absolute magnitudes cannot be determined for many of the bodies, because our knowledge of the parallaxes is very restricted. No detailed study of the total light given by the spiral nebulæ has yet been made.

With regard to velocity, however, the spirals show qualities which distinguish them from all other celestial bodies. Their radial velocities are very large, and, almost without exception, are velocities of recession.[1] Some spiral nebulæ are receding with a speed of more than 600 miles per second, and the average radial velocity is not far short of this figure. The systematic recession is very remarkable, and, like the space distribution of these bodies, has evidently a very deep significance. It is not quite universal—the large Andromeda nebula, for instance, is approaching at the rate of nearly 200 miles per second—but the exceptions are extremely few, and serve

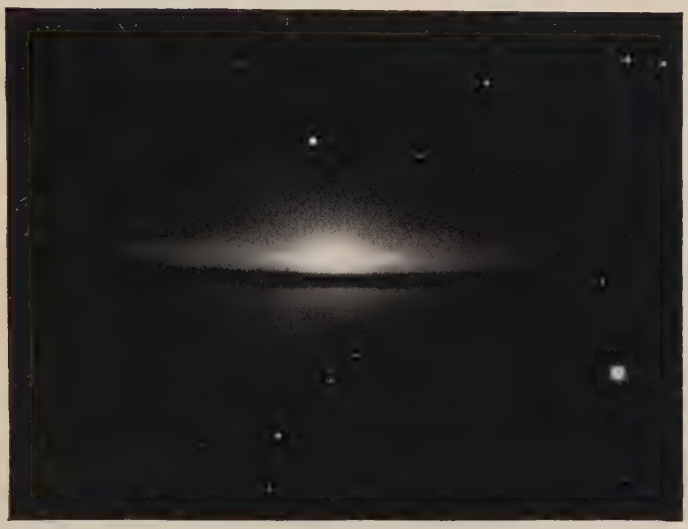
[1] Though we shall here, in accordance with the general view, interpret the observed spectrum-line displacements as Doppler effects indicating recession, it must be borne in mind that other interpretations are possible, *e.g.*, relativity effects. Any conclusion which appears to place the Earth at the centre of the Universe is open to suspicion. It must be observed also that, if the Earth is very eccentrically placed in the galactic system—as is probable—recession from the Earth may in some instances mean *approach* to distant regions of the galaxy.

chiefly to support the assumption that the spectrum line displacements are due to velocity rather than to some unknown influence giving them all the same direction. Van Maanen has recently measured the proper motions of a number of spiral nebulæ by comparing photographs taken at intervals of about ten years. The proper motions are small—they rarely exceed 0″.01 annually, and sometimes are hardly discernible. Curtis, however, from earlier measures of a larger number, found an average annual proper motion of 0″.033. One of the most interesting results of van Maanen's investigation is the detection and measurement of internal motions in the nebulæ. By comparing the relative positions of easily recognised parts of the bodies on the two sets of photographs, he finds that, in all the spirals examined, there is a rotation of the nebula as a whole in a direction which would apparently tend to unwind the spirals, and an outward motion of matter from the nucleus along the arms. The angular displacements corresponding to these internal motions are larger than the proper motions of the whole nebulæ.

Spiral nebulæ are far too distant to allow of the trigonometrical determination of their parallaxes. Various indirect methods have been employed, some based on facts of observation and others on theoretical ideas of the nature of the bodies. Of the former, one of the most interesting depends on the appearance of novæ in the spirals. Within a few years about 27 novæ have been observed to break out in spiral nebulæ—16 of them in the great Andromeda nebula alone. These phenomena, as we know, are otherwise confined to the galactic plane or the Magellanic Clouds. If we can assume that the novæ in spirals are of essentially the same character, and reach the same absolute brightness at maximum, as the galactic novæ, then, from observation of their apparent magnitudes, we can calculate the parallaxes of the nebulæ. The apparent magnitudes at maximum of 30 galactic novæ average +5, and those of the 16 novæ in the Andromeda nebula average +17. If the average absolute magnitude is the same for both groups, then the nebula must be 250 times as distant as the galactic novæ. From very meagre data with regard to the distances of the latter, this result indicates, for the Andromeda nebula, a distance of the order of 150,000 parsecs.

Very much smaller values, however, are suggested by different methods for other nebulæ which, by their angular

PLATE XXXIX.



(*a*)

(*b*)

Elongated Nebulæ. (Non-Galactic)

(*a*) N.G.C. 4594 (Spindle).
(*b*) N.G.C. 3115 (Ovate).

To face page 326.

dimensions, would appear to be still more distant. There are two methods which, in the opinion of van Maanen, deserve greater trust than others. They both depend on the comparison of proper motions with radial velocities in the manner described on p. 111, but the movement considered is, in one method, that of a nebula as a whole, and, in the other, the internal movement of specific parts. It is possible at present to obtain data for the application of the methods only from a few spirals: the results are therefore subject to considerable uncertainty. Such as they are, however, they point to parallaxes between 0".001 and 0".0001. But it is probable that the distances of spirals as a class cover far too large a range to be represented satisfactorily by an average value. If the differences in apparent diameter are due chiefly to differences in distance—an assumption of a kind often made, but perhaps not entirely justified in view of the known enormous range in the diameters of stars—then the small spirals must be at very much greater distances than the larger ones. It must be borne in mind also that parallaxes determined from radial velocities and proper motions are valid only if the velocities of the bodies are distributed indiscriminately between the transverse and radial directions. It is probably fair to assume this with respect to the stars, but, considering the remarkable preference for recession among the radial movements of the spirals, we must view the assumption with much more suspicion when it is applied to them. On the whole, it is probable that no great reliance can safely be placed on present estimates of the parallaxes of spirals.

We know nothing of the temperatures of spiral nebulæ. It is doubtful if there is any meaning in the word "temperature" when applied to one of these bodies as a whole. Whatever they may be, their temperatures probably vary enormously in different parts—so much so that an "effective" temperature would give no indication at all of their actual state. It might be argued that this statement applies also to the stars, yet we determine and make considerable use of the effective temperatures of stars. That is true, but the effective temperatures of stars serve us chiefly in comparing one class of star with another: they are of very little use in the investigation of the structure and character of single stars. And in the spiral nebulæ it appears that the effective temperatures are remarkably alike. The effective wave-lengths, to which they are very closely allied, are found by Lundmark and

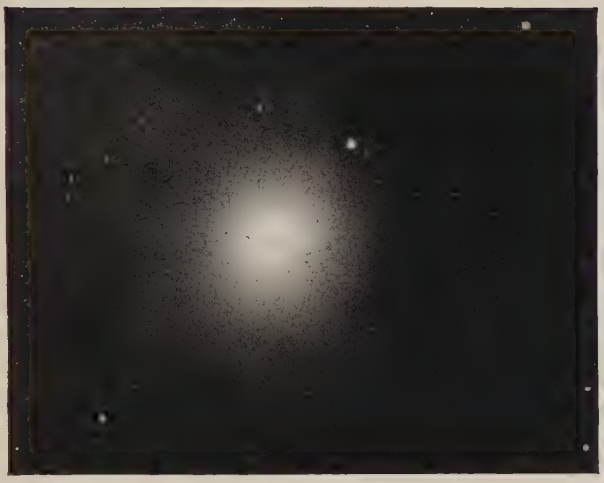
Lindblad to be about λ4290—a value which, if found in a star, would indicate a spectral type *G2*. This is very close to the spectral type of the spirals for which the measurements were made, and indicates that spectral type and colour are related to one another in spiral nebulæ in the same way as they are in the stars.

Very little is known of the masses of spirals, but all the estimates that have been made agree in assigning to them masses enormously greater than those of the stars. The bulk of the evidence, which we shall consider presently, depends on a particular theory of their nature. Oepik, however, has recently formed an estimate of the mass of the Andromeda nebula which, though it involves no definite assumption as to what the nebula is, depends on equally hazardous premises. Assuming the rotation and shape of the nebula to represent the equilibrium state of a gravitating mass, and supposing that the nebula radiates energy at the same rate per unit mass as the Galaxy appears to do, he finds that the mass within 150″ of the centre (the whole nebula has a diameter of 2°), is 4.5×10^9 times the mass of the Sun. The corresponding distance is 450,000 parsecs—much greater than that found for the apparently smaller spirals from the radial velocities and proper motions, and three times as great as the distance indicated for the same nebula by the nova method.

The diameters of the spirals must be very great: until we know the parallaxes, however, we cannot determine them with confidence. Parallaxes determined from radial velocities and proper motions, taken in conjunction with the observed angular diameters of the bodies, indicate diameters ranging from about 3 to 100 parsecs. Larger estimates of the distances would give proportionately larger diameters. Here again it is an open question how far the apparently larger spirals can be considered as representative of all. It may be that the apparently small ones have very much smaller diameters than the objects to which measurements have so far been confined. For the present this question must be left unsettled.

The distribution of spiral nebulæ has already been mentioned: they avoid the Galaxy with remarkable unanimity, and are found, indeed, in greatest numbers round the galactic poles. Shapley has pointed out that the ring-like region in which they are not found is not equally thick

PLATE XL.



(*a*)

(*b*)

Globular Nebulæ.
(Non-Galactic)

(*a*) N.G.C. 3379. (*b*) N.G.C. 221 (M. 32).

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in all directions in the galactic plane. It is thickest in the direction of the constellations Scorpio and Sagittarius, and thinnest in the opposite direction. It appears, therefore, to be in the form of a wedge. This is no less remarkable than the other features of the distribution, and has a bearing on the larger problem of the structure of the Universe as a whole.

Taking a comprehensive view of the characteristics of spiral nebulæ, one is struck by the fact that while, in some respects (*e.g.*, spectrum, colour), they are very similar to the other types of celestial body, there are other features (such as velocity and space distribution) in which they show extraordinary departures from the general rule. It may, or may not, be significant that the qualities in which they are normal are intrinsic properties of the bodies which possess them, whereas the abnormal qualities are relative to the rest of the Universe. We pass on now to a consideration of the theories which have been put forward with regard to the nature of spiral nebulæ and their relation to the other cosmic systems.

The first question that calls for discussion is that of the actual positions of these bodies in space. Are they, or are they not, included within the borders of our galactic system ? It may be said at once that this question has not yet been finally answered. The answer evidently must depend on the dimensions of the galactic system as well as on the distances of the spirals, and since the first of these data is the subject of our next section, we are not yet in a position to consider the whole of the evidence. Without discussing the question further, therefore, we will turn to the views that have been advanced of the nature of the spirals themselves—remarking, however, that some of them presuppose a definite answer to the question we are postponing.

A very consistent and attractive body of evidence has been constructed for the view that a spiral nebula is a universe of stars similar to our own. On this hypothesis, the nebulous appearance, both of the nucleus and of the arms, is an effect merely of distance : the spirals are too far away to be resolved into the separate stars of which they consist. The nucleus corresponds to the flattened stellar system which we dealt with in Chapter XIII., and the spiral arms to our Milky Way, which has been held, on other grounds also, to possess a spiral structure.

It is not our purpose at this stage to discuss the arguments

for and against this hypothesis. It will be sufficient now merely to explain it, and show how it attempts to account for the facts of observation that we have recorded. Beginning with the spectra, then, we have, in the "island universe" hypothesis, a simple and straightforward account of the fact that the majority of the nuclear spectra are of solar type. That is just the type of spectrum we should expect from a vast collection of stars of all types, and is, in fact, identical with the integrated spectrum of our own Milky Way. The relative strength of bright nebular lines in a few of the spectra is not so simply interpreted, but there is, of course, no need to assume that all universes are in the same stage of development, and it is not at all improbable that there are periods in the life of a universe when nebular radiations predominate.

Magnitude provides little definite knowledge on which the theory can test its ingenuity. Seares has shown that the surface brightness of our Universe, as seen from an outside point in the direction of a galactic pole, is less than that of any known spiral—an observation which seems to offer a difficulty to the island universe hypothesis. He concludes that if the spirals are island universes, they must be much larger, or else composed of more or brighter stars, than our own system.

The high velocities of spirals are not at all surprising if the nebulæ are universes distinct from our own. It would perhaps be expected that the relative velocities of separate systems would be greater than the internal motions of one system—though we must remember that van Maanen's observations show that the internal proper motions of spirals are greater than the proper motions of the nebulæ as a whole. The almost universal recession, however, is a stumbling-block to this, as it is, apparently, to every other hypothesis of the nature of spirals. It appears to need a special explanation which has, perhaps, not as yet been given.

Our knowledge of the parallaxes of spiral nebulæ is too uncertain for the island universe hypothesis to offer a definite pronouncement. Obviously, the smaller the parallaxes (*i.e.* the more distant the bodies), the simpler is the problem they present to the hypothesis. The extent of our system must be taken into account in this matter. The occurrence of novæ in the spirals appears to be held to offer no difficulty whatever. There are novæ in our Universe, and it is only to be expected that there will be novæ in others, especially

in view of the observed dark matter, which might very well be nebulous. But on this point it may well be objected that the theory is trying to claim too much. Why should novæ be the only objects in a universe visible as individuals? Their absolute brightness at maximum is very great, it is true, but it is very doubtful whether, in our Galaxy, it is often greater than that of *N* or the brightest *B* and *M* stars. If all such stars in another universe are lost in the general light, why should not novæ be lost also, or, at the best, appear with very much less frequency than they have done in the Andromeda nebula, for instance? In view of this objection it cannot be said that the island universe hypothesis offers a satisfactory explanation of the novæ in spirals.

The relation between effective wave-length and spectral type, on the other hand, is quite in keeping with the idea that the nuclei of spiral nebulæ are aggregations of stars. There is no obvious explanation of the fact that the arms are decidedly bluer than the nuclei, but the meaning of the fact itself is not yet known. The provisional estimates of the masses of the nebulæ show that there is probably sufficient matter in one of them to form a universe, though a direct comparison with our own system is prohibited by the absence of definite knowledge of the mass of the latter. The diameters of the nebulæ also await further investigation, but it appears that if the parallaxes can be explained on the postulates of the island universe theory, the corresponding diameters will present no difficulty.

The space distribution—at least, so far as the avoidance of the Galaxy is concerned—is very satisfactorily accounted for if we suppose our own Universe to be a spiral nebula. Other universes would be expected to occur in all directions in space, and to bear no special relation to the plane of our Galaxy. It is supposed by the advocates of the island universe hypothesis that they do so occur, but that the occulting matter (Plate XLII.), which would be possessed by our own spiral as well as by a large number of others, prevents us from seeing those which lie in the plane of the Milky Way. This is quite consistent with the general character of the observations.

The island universe theory, taken as a whole, is very impressive, on account of the simple way in which it explains some of the most surprising characteristics of the spirals. Allowing for the possibility of certain minor differences

between our own Universe and others, the consistency and scope of the hypothesis are very great. A crucial test, however, is the situation of the spirals in space. They do not appear within the borders of the Galaxy, in the galactic plane itself, but are they, or are they not, outside the nearer limits in other directions? Pending the fuller discussion of this question in the next section, we may point out that it is held by some astronomers that the spirals—or, at least, the nearest of them—are parts of our Universe, and must be considered in relation to the other individual objects there, just as the diffuse and planetary nebulæ are. We will accordingly see how they can be interpreted on this supposition.

If the spirals are within the boundaries of our galactic system, they cannot be regarded as collections of stars unless the stars are very much closer together, even at the borders of the systems, than we have any reason to expect. The largest telescopes fail to show the slightest sign of resolving a single one of them into separate stars. It is held, therefore, by those who regard the spirals as our own compatriots, that the bodies are composed of "nebulous material"—the exact meaning of this phrase having not always been defined. We may think of them as wholly gaseous, or as clouds of dust swimming in a gaseous ocean; either idea would probably explain the appearances and be capable of giving a more or less satisfactory account of the intrinsic properties of the spirals. Their origin and present condition, on the assumption that they are inter-galactic objects, have been the subject of a considerable amount of theoretical investigation, of which we will review the outstanding results.

A dynamical theory of the origin of spiral nebulæ has been advanced by Chamberlin and Moulton,[1] according to which these bodies are the products of stars to which other stars have made very close approaches. Consider a star—which we may imagine to be at rest—subjected to the gravitational attraction of another which passes very close to it. Matter will be drawn from the first star towards the second, and on the opposite side of the first star there will be a similar ejection of matter, just as there are simultaneous tides at opposite points on the Earth through the action of the Moon and Sun. But the disturbing star is moving, and the direction of its attracting force will therefore be changing continuously. The result will be that the matter proceeding from the first

PLATE XLI.

(*a*)

(*b*)

Irregular Nebulæ (Non-Galactic)

(*a*) N.G.C. 3034 (M. 82). (*b*) N.G.C. 4449.

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star will be bent into a spiral form, and detailed investigation shows that the bending of the two protruding arms will be in the same relative directions as that of the arms of a spiral nebula. When the stars have separated so far that their mutual influence is negligible, the arms will be left revolving round the central body, each portion of them moving in an elliptical orbit. The other star will, of course, undergo a similar modification, and it also will be left as a spiral nebula.

This theory, so far as its details can be worked out, offers a satisfactory explanation of the forms of the nebulæ. Obviously, there is room for considerable variation in the shapes of the arms, since there is a large possible range of relative mass, size, closeness of approach, and speed of the affected stars. The spectra also are sufficiently well explained, though we might have expected not quite so pronounced a preponderance of solar type spectra. The high velocities might have been generated by the gravitational interaction of the stars: the systematic recession, however, is no easier to explain on this than on other theories of the nebulæ. There appears to be no necessary reason why the ejected matter should be bluer than the central star. The probable masses and diameters of the spirals are decidedly against the theory, but it is pointed out that the majority of the spirals are apparently extremely small, and might be actually not much greater than stars: moreover, the larger ones might be the result of the process we have described applied to multiple stars, or even stellar clusters. The last-named suggestion, however, is open to the objection that, if the spirals are within the region where close encounters might plausibly be assumed to occur, the separate stars should be visible.

The observed space distribution of the spirals constitutes a very grave objection to the theory. If encounters of the kind imagined take place, it is hardly conceivable that they should do so in all directions but those in which the stars are most numerous and thickly crowded. It is one of the strong points of the collision theory of novæ that both nebulæ and stars are most commonly found in the galactic plane. The assumption of a similar hypothesis to explain bodies whose distribution is exactly the opposite does not commend itself as a very acceptable one. The strength of the theory lies in the natural, and apparently inevitable, way in which

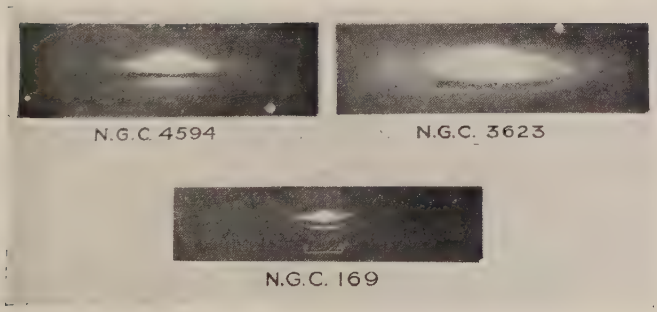
the forms of the bodies are conceived to arise from the hypothetical initial circumstances. Its weakness is the improbability of the initial circumstances in the regions where spirals abound, and the apparently enormous difference in mass and dimensions between the products of the supposed encounters and the observed spirals. It is not at all unlikely that there are miniature spiral bodies which have arisen from the process suggested by Chamberlin and Moulton, but the hypothesis that these bodies are identical with the spiral nebulæ has not met with general acceptance.

Another dynamical theory of the origin of spiral nebulæ, somewhat similar to that of Chamberlin and Moulton in some respects, but differing profoundly from it in others, has been put forward by Jeans. We have already dealt with Jeans's work on binary stars. The principles that were there utilised, if applied to much larger masses of gas, are able to give a satisfactory explanation of spiral bodies. A rotating, contracting mass of gas, as we have seen, must adjust its shape first to the spheroidal and then to the ellipsoidal form, in order to preserve its equilibrium as its period of rotation increases. Thus, if such masses of gas exist, we might expect to find them in these forms. Actually, we find globular and spindle nebulæ, which may accordingly be regarded as contracting gaseous masses.

But now, as contraction proceeds further, equilibrium in any form becomes impossible, and the mass must break up. If it is not interfered with by external forces, a ring of matter will become detached round the sharp edge of the flat ellipsoid which is the last form of equilibrium ; but the further development of the system presents a problem for the solution of which our present mathematical attainments are not sufficient. If, however, the gas is subjected to external forces during its contraction, it can be shown that, instead of the formation of a ring of matter, we shall have an ejection of matter at two opposite points, and the rotation of the mass will cause the arms thus formed to coil round the nucleus. This is the general form of a spiral nebula.

The resemblance extends further. The issuing jets of matter cannot exist long in a continuous stream : they must break up into separate portions. This, according to Jeans, accounts for the condensations observed along the arms of spiral nebulæ. Van Maanen's observations of the rotation of these bodies are quite in keeping with this hypothesis :

PLATE XLII.



Non-Galactic Nebulæ seen Edgewise.

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both the rotation of the nebulæ and the movements of matter outward along the arms are necessary consequences of the suggested mode of origin.[1] The values of the rotation periods afford further evidence pointing in the same direction. The velocities observed by van Maanen in four nebulæ indicate an average period of rotation of about 87,000 years: the individual values range from 160,000 to 45,000 years. Now, in no spiral nebula yet observed is the number of convolutions of the arms greater that about two and a half, so that, if the visible arms form the whole of the ejected matter, we must suppose that the ejection began about 200,000 years ago. This is but yesterday in the history of the Universe: it is hardly conceivable that all the known spirals are of such recent birth. The only alternative is that the issue of matter began much earlier, but that the older ejections have become dark. Here, then, is a simple explanation of the dark matter so often observed around the edges of the nebulæ.

It is possible, on Jeans's hypothesis, to calculate both the parallaxes and the masses of the nebulæ. It can be shown that the average distance between the condensations in the arms of a nebula depends only on the physical and chemical properties of the material of which the arms are composed. Making reasonable assumptions with regard to these properties, a comparison between the corresponding average separation of the condensations and the angle it is observed to subtend at the Earth, gives the parallax of the nebula. The values obtained agree very well with those of van Maanen, determined from proper motions and radial velocities. The corresponding diameters are therefore also in agreement with van Maanen's deductions. The masses are determined by Jeans from the sizes and densities of the nebulæ. The densities of the nuclei can be calculated when the periods of rotation are known, and the measures of van Maanen have enabled Jeans to calculate them for a few nebulæ. For *M*33—a spiral nebula in *Triangulus*—the nuclear density is, in this way, found to be 10^{-17} grams per cubic centimetre. The diameter, according to van Maanen, is about 30 parsecs for the whole nebula, and a little over 1 parsec for the nucleus alone. The mass is therefore about 2×10^{38} grams—equal

[1] Jeans has found, however, that the observed differences in the rates of motion at different parts of the arms cannot be explained in terms of known forces. He suggests a modification of the normal law of gravitation or the existence of some force of which we have as yet had no experience.

to that of about a hundred thousand suns. If approximately the same period of rotation is assumed for all spirals—the four measures of van Maanen suggest that the range is not excessively great—the masses of all of them which can be observed well enough can be determined. On this assumption, Jeans finds that the Andromeda nebula has a mass of 4×10^{40} grams, or twenty million suns.

The known characteristics of spiral nebulæ are not inconsistent with Jeans's hypothesis. There is no reason, so far as we know, why what is essentially an enormous star should not give an ordinary stellar spectrum, though, with our limited experience, we can hardly say that any type of spectrum is impossible for such a body. The magnitudes and surface brightness of the nebulæ likewise offer no test of the theory. The high velocities are not readily explained, nor is the preponderance of movements of recession. If the theory is true, the causes of these things must probably be sought outside it. It is assumed by Jeans that the luminosity is due to high temperature, but it is not possible, from the postulates of the theory, to calculate what the temperatures are. The agreement between colour index and spectral type is natural in stellar material owing its brightness to high temperature. The distribution in space of the nebulæ appears to be a complete mystery: only the island universe hypothesis has so far been able to offer a satisfactory explanation of that.[1] Unless spirals are actually island universes, there must be some special relation between them and the rest of the galactic system which we have not yet been able to discover.

It will be realised that while we have learnt much about the spiral nebulæ, their origin and character are still open questions. We shall return to them later, when we consider the place of the nebulæ in the cosmic scheme. It is impossible to understand the problem adequately without taking into account the other bodies in the Universe. In the meantime, we shall prepare for wider views of the galactic system by calling attention to the clusters and clouds of stars which have, within recent years, almost revolutionised our ideas of its structure.

[1] See, however, p. 390.

CHAPTER XVII

STAR CLUSTERS AND CLOUDS

The association of stars with one another takes place in a variety of ways, which have this feature in common—that they all seem to have arisen mainly from the processes which formed the stars themselves. The solar system, double stars, multiple stars, moving clusters, open clusters, globular clusters, the two star streams, the great galactic system—all of these show characteristics which seem to make it necessary to go back to the origins of the individual stars to account for their association. The stellar world recognises only blood relationships: the bond of friendship, if not quite absent, is so rare as to be beyond detection. A connected group of stars, of whatever kind, has sprung, almost without exception, from a common parent, and now ploughs its lonely furrow, unbroken and perhaps undisturbed, through the unresisting fields of space.

It follows that any system of stars can be dealt with as a unit, whose internal relations, at least for a very long period of time, have been unmodified by external influences. We have seen that double stars, after inconceivably long lives spent in the midst of the galactic system, still show strongly the influences to which they owe their birth. Moving clusters, with members many parsecs apart, still retain the almost absolute parallelism of their motions, so that we can identify them and trace the directions from which they have come. As with single stars, so with systems—each maintains its individuality. It yields, perhaps, to the Universe, playing its part in the great drama of the whole stellar organisation, but it owns the authority of no star or subordinate group of stars.

In this chapter we shall treat of the star clusters—the most complex systems of stars we know, if we except the great flattened structure whose general shape we considered in Chap. XIII. We shall make no assumption here with regard to the intrinsic relation of star clusters to that structure: that will be a matter for subsequent discussion. The individual qualities of the systems, and their distribution in

space, will be the subjects of our present study. We shall deal chiefly with the globular clusters, for in these associations the original bond of union is probably even less modified, and certainly less obscured by alien stars, than it is in the open and moving clusters. Finally, we shall see if we can trace any connection, past or present, between the three types of cluster.

We know at present of 95 globular clusters. Nearly 30 have been discovered within the last five years, so there is a possibility that we shall learn of more before long. The most cursory examination of these remarkable systems is sufficient to show that they form a very important part of the visible universe. They show a peculiarity even in the most obvious of characteristics—their position in the sky. In the first place, they are nearly all in one hemisphere, and the great majority gather round a central direction in that—the direction of the constellations Scorpio and Sagittarius, around galactic longitude 325°. This is an arresting fact. We have had occasion several times to emphasise the importance of galactic latitude, and the distribution therein of different types of star. Galactic longitude, however, has come under notice only once in connection with stellar distribution: namely, in dealing with the occurrence of novæ. And it is very significant that novæ are found most frequently in precisely the same direction—the Scorpio-Sagittarius region—as that in which globular clusters congregate most closely. It would be rash to conclude from this that there is any physical connection between novæ and globular clusters: it is practically certain that there is none. But the fact that they favour the same direction in space is not accidental: it is intimately related, as we shall see, to the general structure of the Universe.

A second noticeable feature of the distribution of globular clusters is their relation to galactic latitude. They are very symmetrically distributed on either side of the central plane of the Milky Way, but they avoid the immediate region of that plane almost completely. Scarcely a cluster is found with a latitude lower than 5°, either north or south, yet at 8°, in each hemisphere, they are crowded most thickly. Their density of distribution decreases rapidly up to 60°, beyond which it is very small. This again is most striking. The clusters are evidently connected in some way with the Galaxy,

PLATE XLIII.

Globular Star Cluster in *Hercules* (M. 13.)

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for it is a plane of symmetry in their distribution, yet they shun the central galactic plane with a remarkable unanimity.

It can be seen, even without actual measurement, that globular clusters are probably very large and very distant. Some of them contain hundreds of thousands of stars, apparently crowded so closely as to be unresolved for some distance from the centre. Their stars, as a rule, appear extremely faint, with apparent magnitudes of the order of 15. On any reasonable supposition with regard to the distances apart and absolute luminosities of the stars, the systems must be accorded enormous dimensions and very small parallaxes. Again, the clusters have no sensible proper motions : this fact also points to great distances. The smaller the apparent size of a cluster, the fainter the stars in it appear to be. This suggests that both these qualities may depend mainly on distance ; otherwise they would scarcely be so closely related. If this is the true interpretation, then globular clusters have a very large range of distance : they cannot possibly be the compact family which their concentration in galactic longitude and latitude might at first lead us to suppose.

These preliminary indications have, within recent years, been subjected to as rigorous an examination as the difficulties of the problem will allow. We owe to Bailey, von Zeipel, and Shapley a very extensive study of globular clusters, the chief implications of which will appear in the next section. The work of Bailey and von Zeipel consisted mainly in cataloguing the clusters and some of their characteristics, such as the number and type of variable and other stars which they found in them. Shapley studied the colours and magnitudes of individual stars in the clusters, and he has drawn from them conclusions which have made it necessary to reconsider the whole question of the structure and extent of the galactic system. His work is still in progress.

One of the chief results of Shapley's work has been the determination of the parallaxes of globular clusters. He has adopted a number of indirect methods—the clusters are all too far away for trigonometrical measures of parallax—the results of which agree so closely with one another that they are accepted by a large body of astronomers. The acceptance is not unanimous, however : we have already pointed out the chief ground of criticism, in connection with the period-luminosity relation among Cepheid variables,

which forms the basis of Shapley's primary method of ascertaining the parallaxes. A number of clusters contain the so-called "cluster" variables, which are now generally recognised as Cepheids. The period-luminosity curve, constructed in the way we have described (p. 252), gives the absolute magnitudes of the variables for which the periods are observed. The median apparent magnitude of the same variables also is observed, and the parallax is then calculated from the equation (p. 80), $M = m + 5 + 5 \log_{10}\pi$.

In many of the clusters, however, Cepheid variables have not been found, but Shapley has indicated another empirical method which, combined with the one just described, is able to give the parallaxes of all observed clusters. In the clusters which contain Cepheids, he has noticed that the mean apparent photographic magnitude of the brightest stars differs from the median apparent magnitude of the variables by a very nearly constant quantity—about $1\frac{1}{2}$ to 2 magnitudes. (Allowance has to be made, of course, for the presence of stars in the line of sight of the clusters, but not involved in them, and Shapley has taken precautions which he considers sufficient to dispose of any error that might have arisen in this way.) The absolute magnitudes of the Cepheids being known from the period-luminosity curve, it follows that the mean absolute magnitude of the brightest stars is known also, because, considering all the stars of a cluster to have the same parallax, the equation we have just repeated shows that the absolute magnitudes will differ by the same amounts as the apparent magnitudes. The results showed the somewhat remarkable fact that the mean absolute photographic magnitude of the brightest stars in a cluster was practically the same for all clusters containing Cepheids—about -1.51. Assuming, now, that this is true for *all* clusters, whether they contain Cepheids or not, it is necessary only to measure the apparent magnitudes of the brightest stars in order to determine the parallax of any cluster.

A method of this kind is too heavily burdened with a somewhat risky assumption to carry conviction on its own shoulders. It may, however, give very valuable help to other methods if they point in the same direction. We have already mentioned that the apparent diameters of globular clusters are related to the magnitudes of the stars in such a way as to suggest that the differences in these quantities in different clusters arise mainly from the differences in distance

of the clusters from the Earth. Assuming, then, that all clusters have the same actual diameter, Shapley was able to calculate the relative parallaxes from the apparent diameters, and hence, knowing the parallaxes of some clusters from the periods of Cepheid variables, he arrived at the absolute parallaxes of all clusters. The results he obtained agreed closely with those found by the previous method. Here again, special precautions had to be taken to determine what was the actual boundary of a cluster—not a simple matter when dealing with a system thinning out gradually in a field containing many extraneous stars.

Shapley has employed yet another method, which seems to have greater inherent validity than any of the others. He has been able, with the large Mount Wilson instruments, to determine directly the spectral types and colour indices of a considerable number of stars in the brighter clusters. He finds a wide range of values in these and other characteristics, which suggests strongly that stars in clusters are of the same general types as the stars in our own neighbourhood. For example, there is a range of at least nine magnitudes between the brightest and faintest stars observed; the colour indices range from -0.5 to $+2.0$ (cf. p. 121); and there are stars of spectral types *B*, *A*, *F*, *G*, *K*, *M*, whose relations to the respective colour classes agree with the corresponding relations for our neighbouring stars—an indication, as we have shown, of the absence of appreciable light-scattering in space. It is therefore a most reasonable assumption that the absolute magnitudes of cluster stars are the same as those of our catalogued stars of the same spectral types. If this is so, then we have the necessary data for determining the parallaxes of the clusters. The obvious question arises: Are the stars in clusters giants or dwarfs?—for we know that the absolute magnitudes of these two types of star are, in general, quite different, so that the spectral type alone is usually not sufficient to indicate the absolute magnitude of a star. In our present problem, however, we can evade this question by confining our attention to the *B* stars, among which the giants and dwarfs merge into a common absolute magnitude. By assigning to the *B* stars in clusters the average absolute magnitude of the *B* stars in our own region of space, Shapley had deduced parallaxes for the brighter clusters which are again in agreement with those found by the other methods.

It is to be noticed that the comparison of the two classes

of *B* star has an additional justification in the fact that it is a comparison of cluster stars with cluster stars. We alluded in an earlier chapter to the strong tendency of *B* stars to form moving clusters, and we shall presently show that the whole system of local *B* stars probably outlines a more comprehensive cluster in the neighbourhood of the Sun. There is reason to believe that the difference between globular, open and moving clusters is not very fundamental—that it is, in fact, mainly a matter of position in space and distance from the point of observation. The selection of *B* stars for the comparison is therefore a particularly happy one.

At the same time, it must be pointed out that there is a certain amount of risk in assigning to these stars the same average absolute magnitude as that of neighbouring *B* stars. When the brightest stars of other types are considered, it appears that in globular clusters there is a steady decrease of brightness in going from the *M* towards the *B* type. We are bound to assume that the brightest stars are giants, so that, in clusters, the constancy of brightness throughout the giant stage, which we have observed in the galactic system, and which Eddington has explained theoretically, seems not to hold. Eddington has accounted for this by supposing that stars in a cluster, unlike those outside, were all born at about the same time. The least massive will then have advanced farthest, so that there will be a steady decrease of mass from *M* to *B* along the giant sequence. Since the absolute magnitude should theoretically be the same only for giant stars of the same mass, the abnormality of cluster stars is explained. It is an apparent abnormality, not of physical behaviour, but of relative age. But, whatever the explanation may be, from our present point of view this peculiarity of cluster stars makes a comparison between cluster and galactic stars somewhat ambiguous. If the *B* stars have identical absolute magnitudes, the *M* stars are brighter in the clusters: if the *M* stars have identical magnitudes, the *B* stars are fainter in the clusters. There is no certain means of deciding how the comparison is to be made, but there is this to be said in favour of identifying the absolute magnitudes of the *B* stars—that the characteristics of *B* stars seem to be attainable only by stars which are more massive than the average galactic star. No comparison which gives to these stars an absolute brightness much below that of zero magnitude can be defended on physical grounds.

These considerations are important, because Curtis has questioned the assumption that the stars which we photograph in clusters are mainly giants. He points out that, in any region of space in our own neighbourhood, the dwarf stars greatly outnumber the giants: it is probable, then, that they will do so in clusters. In the absence of *a priori* knowledge of the absolute magnitude of any star in a cluster, the safest assumption to make is that the average absolute magnitude of cluster stars is equal to that of stars around the Sun. The argument is supported by the character of the composite spectra of clusters and of the Milky Way. Fath has shown that the spectra of the general light of these bodies, undifferentiated into separate stars, are of type *F-K*—say, of solar type. Taking the Milky Way as representative of the galactic system, it follows that the average stars, in clusters and in our own neighbourhood, have the same spectral type: their average absolute magnitudes should therefore be the same. Now the average absolute magnitude of solar-type stars in the galactic system, taking giants and dwarfs in the proportions in which they occur, is not brighter than +6. If we assign this value to the average solar-type cluster star, we derive a distance for the cluster only one-tenth of that given by Shapley. The weak point of this argument seems to be that it ignores the fact that, in a distant system, the majority of the stars *detected* will be giants, because most of the dwarfs will be too faint to be seen. The comparison of averages, therefore, is a comparison of two sets of heterogeneous data. It is difficult to see how such a comparison can have greater validity than an argument based on the observed spectral types and colour indices of individual stars.

As examples of the parallaxes obtained by Shapley, we may take the nearest and the farthest of the clusters—ω Centauri (see Plate II), and N.G.C. 7006, respectively. The parallax of ω Centauri is considered by Shapley to be rather uncertain, because of the provisional character of the estimated magnitudes of the variables and bright stars: the figure proposed is 0″.00015, corresponding to a distance of 6500 parsecs. Three methods of determination of the parallax of N.G.C. 7006 give the respective values, 0″.000015, 0″.000014, 0″.000016; the distance is taken as 67,000 parsecs. These figures far exceed any previous measures of distance of bodies in space, and it is not surprising that their accuracy

has been challenged. The corresponding diameters of the clusters also are very great. *M*3, for example, (Plate XLIV), which is estimated to be 13,900 parsecs from us, would have a diameter of about 144 parsecs ; light would take 470 years to cross from one side to the other. Sirius, if placed in the cluster, would appear 11 magnitudes fainter than a star just visible to the naked eye. The distance between the closest pairs of stars shown in the figure is half the distance between the Sun and α Centauri : the Sun itself would be too faint to appear in the photograph. At the centre of the cluster, the stars are crowded together much more closely than they are in our region of space. In the circle drawn on the figure there are at least 15,000 stars brighter than apparent magnitude 20, as against only 4 or 5 within the same limits of magnitude in an equal sphere round the Sun. These enormous systems might be veritable universes in themselves.

With regard to individual stars in clusters, it is assumed that, in the main, they are similar to the stars near at hand. We have seen how, in his parallax investigations, Shapley has made use of this assumption in two forms—in deducing absolute magnitudes from colour indices and from period of variation of Cepheids, respectively. We are bound to suppose that clusters contain late type dwarf stars as well as giants, though, owing to their great distance, they cannot be detected : the range of nine magnitudes, which has actually been observed, is as much as could be expected. The earliest variables to be found in clusters were the Cepheids, the most luminous ; but a number of irregular variables also have been discovered among the bright red stars. It is very difficult to decide how far the analogy between cluster and galactic stars can be pressed. We know that it breaks down in the statistical study of absolute magnitudes of giant stars of different types, and, apparently at least, the average period of the Cepheid variables is smaller in the clusters than in the Galaxy. From his study of the clusters, Shapley has been led to the conclusion that the absolute photographic magnitude −0.2, which marks the lower limit of brightness of Cepheid variables (see Fig. 24), is a magnitude which characterises an excessive number of giant stars. He suggests that when a cluster giant star reaches this magnitude it undergoes no change of brightness for a considerable time, although its density continues to increase (witness the range of period of Cepheids at this magnitude, remembering that period is

PLATE XLIV.

Globular Cluster M. 3 (N.G.C. 5272).

(The small circles enclose typical variables ; the small square, a special variable. If the Sun were situated at the centre, the Hyades would be at the position marked near the bottom of the figure.)

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probably determined by density). Whether this phenomenon, if it is real, extends to galactic giants as well, remains to be seen.

There is one point connected with the conception of the "average star" in a cluster that may prove to be useful in distinguishing between clusters and nebulæ in the identification of very faint objects. It has been mentioned that Fath's integrated spectra of the clusters belong approximately to type *F*. Lundmark confirms this observation by an examination of colour indices. He finds that four globular clusters have each an integrated effective wave-length of about 4190—a value which seems to be characteristic of clusters: it corresponds to spectral type *A*8. Nebulæ have a distinctly different effective wave-length. It may, therefore, be possible to decide the character of a faint extended object by an examination of its colour. It must be assumed, of course, that absorption of light in space can be ignored—an assumption we have already shown to be justified, and one, moreover, which is still further supported by the agreement between the spectral types indicated by the effective wave-lengths and the direct observations of Fath.

In the study of the internal structure of globular clusters, only a beginning has been made. It is obvious, of course, that the systems are symmetrical about their centres, and gradually thin out in all directions. They appear to be spherical in shape, but careful investigation shows that this is often an illusion. By counting the stars in each of a number of sectors radiating from the centre, Shapley and others have shown that a globular cluster, in general, is somewhat flattened; *i.e.* that it possesses a "galactic plane." Many thousands of stars must be counted, however, before this becomes evident. The clusters are presented to us at various angles, so that the ellipticity is more pronounced in some than in others. The flattening, however, is never so pronounced as it is in our own galactic system. The clusters which appear to be quite spherical may be flattened systems whose short axes point in our direction: this interpretation is supported by the fact that these clusters appear to be less condensed at the centre than do the elliptical ones—a natural consequence of a line of sight passing through the narrowest part of the system. A curious feature of the observations is that there is a general tendency for the clusters nearest to the Milky Way to lie parallel to our galactic plane.

In the nearest clusters, which can be studied in more detail than the majority, Shapley finds evidence that the *B*-type stars and Cepheid variables are concentrated towards the galactic planes, just as they are in our own system. This is a very interesting result, for it suggests that our Universe of stars is a globular cluster, similar to many others which we study from a detached point of view. The suggestion, however, is probably untenable, as we shall see. The marked symmetry of distribution of the clusters with respect to our galactic plane is apparently inexplicable if that plane has no special significance in connection with the globular clusters as a body—which it could not be supposed to have if its direction were the result of accident. There are other reasons also for assigning a special rôle to our own galactic plane. Nevertheless, the existence of galactic planes in large aggregations of stars is in itself important, and suggests that a somewhat flattened shape is a necessary condition of equilibrium in such systems.

Slipher has measured the radial velocities of 15 globular clusters. He finds that the average speed is about 78 miles per second, with a range extending from zero to 256 miles per second. The velocities are therefore much greater than the average velocities of stars, and give further evidence that the clusters are very remote ; for if the cross velocities are at all comparable with the radial ones, the absence of observable proper motions can be attributed only to great distances. But perhaps the most interesting result of Slipher's observation is that only 4 of the 15 clusters are receding from our region of space. Globular clusters seem to be approaching the galactic plane at a rate which would bring them within the Galaxy in a period of time short compared with cosmic periods. It is difficult not to suppose that this has been going on in times long past, and yet the galactic plane is totally free of globular clusters. How can this be explained ?

Shapley has proposed a very interesting theory, which has much to recommend it. He suggests that when a globular cluster enters the galactic system, the gravitational forces it there encounters tend to break it up. The quickly-moving, outlying stars will be the first to go, and will be lost to the cluster altogether. The remaining members, though their separation from one another is increased, retain a considerable measure of association, and become, in fact, an open cluster. According to this idea, the open and moving clusters in the

galactic system are the remnants of former extra-galactic globular clusters.

From this point of view, the distribution of open clusters is very significant. They are found in far greater numbers in the galactic plane than outside it : their region of abundance is the region which the globular clusters avoid. In galactic longitude, too, they are most numerous in the opposite direction to the place of concentration of the globular clusters. The two types of system appear to shun one another, as if each were unable to survive in the conditions most suitable for the other. Shapley finds that the distances of open clusters are much smaller than those of the globular clusters—another indication that the former are actually entangled in our own body of stars. Their members are much less numerous, and are considerably farther apart, than are those of the globular systems.

There seems to be some dynamical justification for the view that globular clusters are the parents of the open ones. The details of the process by which the transformation has taken place present far too intricate a problem for our present mathematical powers, but the general result is quite consistent with the findings of mathematical analysis. Jeans has shown that the mutual gravitational attraction of two groups of stars would tend to open them out : he points, in fact, to other evidence that our own system was once more compact than it is now. If, then, a globular cluster entered the Galaxy, it would probably become an open cluster. This result, taken in conjunction with the distribution in space of open and globular clusters, forms a very strong argument for Shapley's suggestion.

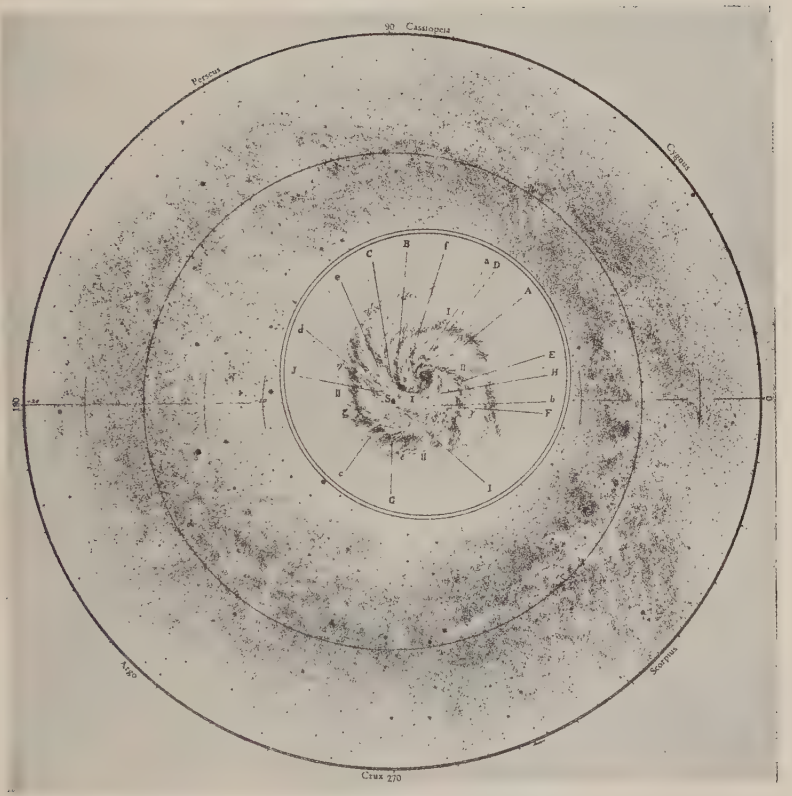
The study of clusters can scarcely fail to raise in our minds the question : What is our own galactic system ; is it a super-cluster, similar in all essential characteristics to other clusters, but much larger, or is it the backbone of a wider universe, comprehending clusters and everything that we observe ? We must postpone the complete discussion of this question to the last section : it belongs to the study of the Universe as a whole, which, in this chapter, we have brought within sight, but which we must for the time keep at a distance. It will be appropriate, however, here to consider the character of the Milky Way—the broad, irregular belt of faint luminosity that encircles the sky and defines the plane which we have

found to be so intimately associated with almost all stellar phenomena. We have seen that the stars as a whole are very strongly condensed to the Milky Way, and the difficult question naturally arises: Is the Milky Way only an appearance due to the very great depth of stars through which we look in its plane, or is there, beyond the stars, a Milky Way proper, which is distinct from, but co-planar with, the flattened universe of stars? Several attempts have been made to answer this question.

First of all, let us see clearly what the appearance of the Milky Way is. The belt of luminosity which encircles the heavens varies along its length in almost every one of its characteristics; width, luminosity, degree of condensation, character of outline—all these qualities change from place to place along the structure. Under high magnification, the Milky Way is seen to consist mainly of innumerable hosts of stars, showing no regularity of arrangement, but apparently heaped together in dense clouds (see Plate XLV). Here and there rifts appear; some of them arise, perhaps, from dark, obscuring matter between the star clouds and the Earth, but others, apparently, are actual vacant spaces. One enormous rift, extending for more than 50° between the constellations Cygnus and Aquila, divides the Milky Way into two distinct parts; another large gap occurs in the southern sky in the constellation Argo. From a width of less than 5° in Taurus, the Milky Way expands about fivefold between Orion and Canis Major, the neighbouring constellations. In the direction of Scorpio and Sagittarius—the region of the globular clusters—the stars are too thickly crowded to be distinguished individually (see Plate XLVI): in the opposite direction, they are perhaps crowded most thinly.

The Milky Way, it seems, possesses contradictory properties. By its unbroken, plane ring-like shape it suggests a unity of structure, and immediately attempts to dispel the suggestion by showing as much irregularity in detail as it possibly can. It will be obvious that, with such a structure as this, the "central plane" of the Milky Way, to which we have referred on several occasions, is not at all clearly defined. Nevertheless, such a plane can be selected, and it appears to trace a circle in the sky of which we are not quite at the centre. The solar system is situated, in fact, a little north of the central plane of the Milky Way. It is this central plane that defines the fundamental circle of the galactic system of co-ordinates.

PLATE XLV.



Photographic Chart of the Milky Way.

(The Spiral shown in the centre represents the appearance of the Milky Way from an external viewpoint, according to Easton.)

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Galactic latitude is the angular distance north or south of the plane (*i.e.* within the galactic hemisphere containing the north or south celestial pole), and galactic longitude is the angular distance along the plane from the point at which it meets the celestial equator at right ascension 18 h. 40 m., the direction of increasing longitude being from the northern to the southern part of the circle.

The galactic plane is a mathematical conception, extremely important in the study of the structure of the Universe, but telling us nothing about the physical character of the Milky Way. We must now consider whether there is actually a Milky Way, or only a flattened system of stars. About ten years ago, the opinion was prevalent that, beyond the universe of stars, there was an encircling system of irregular star clouds which gave the appearance of the Milky Way. The system might, or might not, be continuous with the inner oblate system of stars—that was uncertain ; but the two structures were evidently part of one physical universe, or they would not be co-planar. There was much to support this view. The irregular form and density, and the apparently sharp outline, of the Milky Way all pointed to local stellar aggregations rather than to a uniform distribution of stars having a great depth. Moreover, the existence of the Magellanic Clouds—two objects in the southern heavens, far removed from the Milky Way yet possessing all its characteristic features—seemed to make it impossible to doubt that the Milky Way was a definite structure distinct from the stars which formed the data of statistical investigations.

The idea that the Milky Way clouds lay beyond the main stellar system rested on no definite grounds. The regularity with which the star density decreased with distance from the Sun (see Chap. XIII.), suggested a symmetry for the inner system which was inconsistent with the existence in it of shapeless local aggregations. Tentative estimates of the distance of the nearest parts of the Milky Way were not irreconcilable with this view. Nevertheless, it was held by some that the galactic clouds had no special distribution in space, but were scattered indiscriminately in a single plane. The " inner oblate system " was, from this point of view, looked upon merely as one of the clouds in which we happened to be. The lack of observed symmetry in the internal structure of the other clouds, however, was unfavourable to this idea, which, notwithstanding, had points in its support.

Easton, whose photographic chart of the Milky Way is reproduced in Plate XLV, undertook extensive studies of the structure of the galactic clouds. He put forward the hypothesis that they form a spiral system (illustrated by the drawing in the middle of Plate XLV) whose centre is in the direction of the constellation Cygnus. He gave a detailed account of the appearance of the Galaxy regarded as a double-armed spiral —a view which evidently simplifies the explanation of the two branches formed by the rift to which we have referred. The idea is of great interest, in view of the likeness which it draws between the stellar universe and the spiral nebulæ.

But lately, a new conception of the Milky Way has arisen, as the result of Shapley's investigations of star clusters. The open clusters, as we have seen, are strongly concentrated to the galactic plane, and, from a study of the galactic fields surrounding a number of these clusters, Shapley has been forced to the conclusion that the depth of the star-populated regions in the line of sight is much greater than it has previously been thought to be. Hitherto, the evidence with regard to the relation of colour index to apparent magnitude has indicated the almost complete absence of faint blue stars: thus, Seares found a steady reddening of stars with increasing apparent magnitude, in more than one region of the sky (see p. 143). This has been interpreted as an indication of the limit of the Universe: a blue star, even at the boundary of our system of stars, would appear too bright to be classed with the stars of highest apparent magnitude. But, in the galactic clouds, Shapley has found blue stars ranging continuously from apparent magnitude 13 to 15 or fainter. This indicates great distance and great continuous depth of the clouds. This, in itself, would not be inconsistent with the idea that the galactic clouds surround the stellar system: it would simply mean that they were much farther from it than earlier investigators had supposed. But Shapley has shown that, in all probability, the stellar system extends up to the clouds and even beyond them. There are Cepheid variables, for instance, which appear to be more distant than even the galactic clouds. He has, therefore, put forward an alternative hypothesis.

According to Shapley, the appearance of the Milky Way is mainly due to great star depth: there is no ring or spiral of star clouds outside the stellar system. But, at the same time, the uniform, symmetrical system, which was formerly

PLATE XLVI.

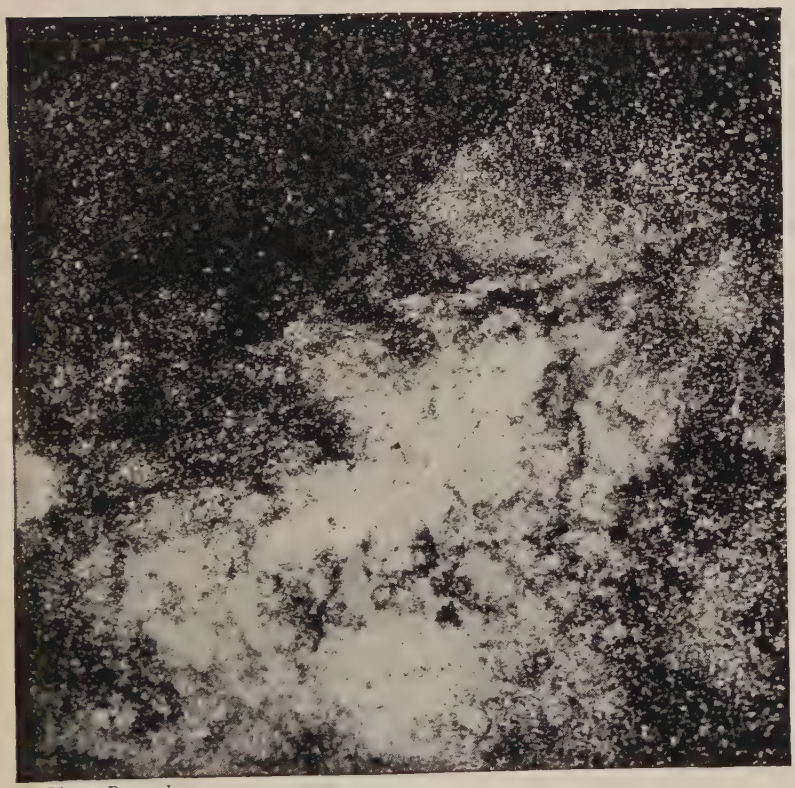

Photo: Barnard.

Great Star Cloud in Sagittarius.

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imagined to form, with the Milky Way, the whole of the sidereal universe, is now relegated to a comparatively small region in the neighbourhood of the Sun. It is not that which provides the appearance of a Milky Way. The star depth is due to an irregular, heterogeneous, plane distribution of stars, grouped together with large variations of density, in which the stars in our own neighbourhood play a subordinate part. The larger system thus conceived is not limitless: the Sun, in fact, occupies a very eccentric position in it, and that is the reason why the Milky Way appears so much more condensed in the Scorpio-Sagittarius region—which is in the direction of the supposed centre of the system—than it does in the opposite direction. The irregularities of structure and the large rifts in the Milky Way are natural consequences of a chaotic assemblage of bodies containing more or less organised clusters, formless clouds, and isolated stars.

Whether this view will survive after further investigation remains to be seen. It forms part of an even more comprehensive conception of the Universe which we shall describe in the next section. It offers at least a natural explanation of the main features of the Milky Way. One great difficulty, however, remains: what are the Magellanic Clouds? Before leaving the discussion of star groups, we must say a few words about these remarkable objects.

The two Magellanic Clouds, the Greater and the Lesser—*Nubecula Major* and *Minor*, as they are sometimes called—are probably the most mysterious objects in the sky. They are perhaps best described as portions of the Milky Way, exiled from the galactic plane to regions far from their kind. The bodies which show galactic concentration most strongly are to be found in the Magellanic Clouds—some of them nowhere else except in the Milky Way itself. Star clusters, planetary nebulæ, Wolf-Rayet stars, novæ, Cepheid variables—these are found in profusion in the Magellanic Clouds, and, which is very remarkable, there are also nebulæ which appear to be spirals—the one class of object which, without exception, avoids the Milky Way. Hinks has maintained that these nebulæ are not typical spirals, but are objects of quite a unique kind: whether they are spirals or not, however, their existence is important as a mark of distinction between the Milky Way and the Clouds.

The radial velocities of nebulæ in the Magellanic Clouds

indicate that both of these objects are receding from us very rapidly. In the Greater Cloud, twelve of the so-called spirals show an average radial velocity of 175 miles per second—a value in close agreement with a separate determination from the planetary nebulæ. The planetary nebulæ in the Lesser Cloud indicate a radial velocity of 100 miles per second. Although the nebulæ, no doubt, have motions of their own within the Clouds, it is a fairly safe assumption that their average velocities are not far from the velocities of the Clouds as a whole. These velocities are very high—of the same order as those of the globular clusters. The distances of the Clouds are, according to all estimates, very great. From the apparent diameters of the globular clusters in the larger Cloud, Shapley has deduced a parallax of 0″.000029, by the method already described; this implies a distance of 35,000 parsecs. If this is accurate, the diameter of the Cloud must be 4500 parsecs, and the system is larger than any known unit except the galactic universe itself. The distance of the smaller Cloud has been estimated separately by Hertzsprung, Kapteyn, and Shapley, from observations of the apparent magnitudes of Cepheids whose absolute magnitudes were revealed by their periods. The resulting parallaxes range from 0″.0001 to 0″.00004, corresponding to a mean distance of 19,000 parsecs. The diameter calculated from this figure is 1500 parsecs, so that the Cloud is smaller than its companion in fact, as well as in appearance.

These dimensions are of the same order as those which, until recently, were ascribed to the galactic system itself. The extent of the galactic system, however, through the researches of Shapley, has recently grown tremendously in estimation, and the Magellanic Clouds must now probably be looked upon as systems similar to the galactic clouds which, for some reason, lie outside the general galactic plane. Before their distances and dimensions were more than guessed at, other views of their nature were entertained. They give indications of possessing a spiral form, and it was held by some that they were spiral nebulæ which happened to be much nearer to us than the majority of these bodies. This view is consistent with the idea that the spiral nebulæ are island universes, but it cannot be reconciled with the suggested distances of the nearest typical spirals. On the other hand, the idea prevailed in some quarters that the Magellanic Clouds were universes like our own, but that the spiral nebulæ were

of a totally different character—probably of the same nature as the diffuse nebulæ. If this is so, we must now be prepared to regard them as very much smaller universes than ours, and similar in character to what we believe to be parts of our own Universe. Since Shapley's proposed dimensions for the galactic system and the estimates of the sizes of the Magellanic Clouds are derived from the same principles, we can hardly doubt that their relative values are accurate, whatever may be said of them as absolute measurements. The most probable explanation of the Magellanic Clouds, therefore, is that they are systems of the same kind as some of those which build up our Galaxy. It may be that they once inhabited the same regions, but, from some cause or other, left the populous plains and took wing for the southern skies. If their present velocities are even approximately equal to those which they have had in the past, they must have been associated with the larger system at no very remote time, as cosmic history runs. This, however, is a speculation: what we actually know about the Magellanic Clouds is that they resemble the aggregations of the Milky Way, with two striking exceptions—they are not in the galactic plane, and they contain a type of nebula which is not found there. Perhaps these two phenomena are not unconnected.

The study of groups of stars is, at the same time, a prologue and an epilogue to the study of the structure of the Universe. We are led naturally from the consideration of large organised systems to the problem of the largest system of all—if, indeed, the Universe can properly be called a system. But, also, we cannot truly understand the smaller groups without knowing something of their relations to the whole. The reciprocal relations of the one and the many form perhaps the chief reason for the necessary lack of logical sequence in any intelligible account of modern astrophysics. Through the study of the stars around the Sun, the plane of the Milky Way was invested with a new importance: it defined the shape of the Universe, and provided a standard with reference to which the distribution of all objects would henceforth be described. The study of globular clusters gave to the galactic plane a still greater dignity: from the plane of concentration of the local stars and the Milky Way clouds, it became the outstanding feature in a universe whose dimensions were of a magnitude hitherto undreamed of. What now of the local

distribution, the step by which it rose to power? With an ingratitude not unknown in human relations, it turns on the local system and says [1]: You are not of me; you define a plane, close indeed to mine, but not the same; you outline, not the Universe, but merely your own microcosm, which I rule but to which I acknowledge no obligation; the secrets of Nature are in my keeping, your day is done.

But the local system may yet reassert itself. Nature is whimsical, and may be caught peeping through the windows of her Universe while the turrets are sought in vain. With her there is no last or first. We close this survey of Astrophysics with a discussion of the Universe as a whole because it marks the beginning of a new endeavour, not the crowning-point of the old. When we have sounded the Universe and learnt its laws; when we have tracked space to its ultimate distances and pierced the veil of time, we may—though the hope is somewhat daring—begin to understand the Earth.

[1] See page 372.

Section IV

THE UNIVERSE

CHAPTER XVIII

AS IT IS

WHAT is " the Universe " ? Is it a whole or an aggregate of disconnected parts ? Has it an infinite extent and content, or has it limits ? These are transcendental questions, which will always be asked and never answered ; for the human mind, " born too great for its ends, never at peace with its goal," is doomed ever to seek for that which it has not the power to attain. We cannot escape the search : ignore as we may the ceaseless impulsion, it is still behind us, directing our footsteps to the endless ways. Art treads the paths with happy feet, forgetting the unattainable in the beauty of the way. Philosophy strains piercing eyes for the end that does not appear. Science turns to the cul-de-sac, only to find the limit receding before her feet as she advances.

What, after all, is so-called " exact " science ? The experimenter shuts himself in his laboratory and, armed with scales and balances, measures the fluctuations of phenomena within a " finite space " and a " finite time." He finds that when so much of A vanishes, so much of B appears, and he announces a discovery. By a rigorous exclusion of external influences, an exact law of Nature has been revealed. But has it ? Who is to say how much of A has vanished and how much of B has appeared ? A prominence leaps up from the Sun, and the gravitational field in the laboratory is changed : the Sun has crept into the experiment. A hundred thousand years ago the electrons in a star vibrated, and, during the experiment, the electrons in the " confined space " leap in sympathy with their unknown comrade : an answering vibration is sent out—whither ? The limited region, free from external influences, is open to the Universe. The discovery of the law of Nature is merely a tribute to the imperfection of our measuring instruments. Granted perfect means of measurement, we should not establish a single principle without a grasp of the Universe.

The everlasting dilemma of Science is that it cannot be

satisfied without the Universe, and cannot work with it: the Universe is, at the same time, a necessity and an impossibility. Hence the need to formulate a plan of the world as we know it, and the essentially unsatisfactory character of the plan when it is made. In this, the final section of our book, we shall attempt a summary of present ideas regarding the inter-relations of the bodies whose properties we have already discussed. We may say at once that there is no general agreement on this matter. The data are so scanty compared with the vastness of the problem that the conclusions reached by any thinker must owe almost as much to his temperament as to his logical powers. Again, the Universe with which we are to deal is that which our instruments reveal to us; it is not the totality which rules the events of all places and all times. Our Universe is a growing thing, which perhaps will be made larger by every advance in telescope construction. The real Universe, which, though possibly infinite in extent and duration, yet enters into and controls the simplest experiment, so that the present moment is the heir of all the ages, and the ground on which we stand is the focus of all the distances—that Universe is perhaps not yet glimpsed. We must reconcile ourselves, at the outset, to a very humble achievement.

Very arbitrarily, we have divided the discussion into three parts—divisions of a time scale. We begin by considering the Universe "as it is." But—such is the transcendental nature of the problem—we at once meet the difficulty that we can never know the Universe, even the fragmentary aspect of it to which we are forced to limit ourselves, as it is. No man hath seen the Universe at any *time*, nor ever can do. Let us suppose that, for practical purposes, we may speak of our view of the Earth as a representation of what the Earth is at the present moment. Then we must wait $8\frac{1}{2}$ minutes before we can say how the Sun appeared at the same moment, because the light of the Sun takes that time to reach our eyes. At nightfall we look into the sky and see the star Spica, but not Spica "as it is": for that 180 years must elapse, and for the fainter stars around we must bequeath the comparison to generations yet farther ahead. "In Virginè the sultry sun 'gan sheen," sang Chatterton, but we have not yet seen the constellation Virgo as it was when the "Excelente Balade of Charitie" was supposed to have been written. We turn our telescopes through a few degrees and see a globular cluster.

Tens of thousands of years must pass before the present state of that cluster can be known on the Earth.

If we could, in a single glance of the eye, embrace a view of the whole visible Universe, we should simultaneously be looking into the distant places and the distant times. Time and space are inseparable. When we speak of the Universe "as it is," we must perforce mean the Universe as we see it now. The system which, in this chapter, we shall attempt to describe is included in a sphere whose centre is in the Earth and whose radius is the distance of the farthest object we can photograph. The nearer to the centre an object is, the nearer to what we call the present time is the instant at which it had the aspect we see. For the Universe as it is we must understand the Universe as it appears; for the Universe as it was, the Universe as it would have appeared ages ago had there been any one on the Earth to see it: for the Universe as it will be, the Universe which future generations, if the Earth holds them, will know. The history of the Universe is inseparably bound up with human history.

It should, nevertheless, be pointed out that, so far as general shape and configuration are concerned, there is probably very little difference between the Universe we see and the same Universe corrected for the time factor. The relative movements of the heavenly bodies are so exceedingly slow compared with the velocity of light that their changes of position while light is passing between them are almost negligible compared with their distances apart. The importance of the time factor appears when we compare the apparent physical states of near and distant bodies: we are comparing them at different ages. It appears also in the consideration of catastrophes, such as the formation of novæ; and, less certainly, in connection with the direction and magnitude of velocities, which may have changed since the bodies sent out the light by which we measure them. But the general *structure* of the Universe must be almost the same as it appears, provided there is no unsuspected error in our observations and deductions, and in this chapter we may therefore, without a serious distortion of facts, regard the Universe we are investigating as the Universe "as it is."

There are two ways of attacking the problem of the structure of the Universe: we can start from the centre and work outwards, or start from the boundaries and work inwards.

Both methods have been adopted, with differing results. Perhaps this is no other than could reasonably be expected. We have only just reached a position from which the problem can be faced at all, and the pioneer attempts to solve it can hardly be other than the roughest approximations to the truth. We make, therefore, no attempt to give a single, consistent view of the Universe. We can only indicate the results of the two methods—the first as applied by Kapteyn, and the second by Shapley—and leave the question of their reconciliation in abeyance.

In Chap. XIII. we saw that, as the result of counting the stars of different apparent magnitudes, the stellar universe was found to be a flattened system in which the star density diminished with increasing distance in all directions. This general conclusion was valid, notwithstanding the fact that the absolute magnitudes of the stars were not taken into account, so long as it could be held that there was a finite brightness not exceeded by any star—an assumption which we have no reason to question. If all the stars had the same absolute brightness, the figures given in Chap. XIII. would represent the actual structure of the Universe—that is, the star density in any region of space covered by the observations. But since, as we know, the absolute magnitudes of the stars vary over a very wide range, which apparently bears no simple relation to the positions of the stars in space, this simple solution of the problem is not possible. It is necessary to find out how the stars of different magnitudes are distributed with respect to their distance from the Earth.

The requisite data can be obtained from more than one set of observations. Kapteyn, in his last work, used counts of stars of different apparent magnitudes, and the empirical relation between apparent magnitude and parallax referred to on page 112, which was derived from the average parallaxes obtained from the υ-components of proper motion (p. 110). From these data he was able to calculate expressions for two quantities—first, the total number of stars (N_m) projected on a given area of the sky, having apparent magnitudes between $m+\frac{1}{2}$ and $m-\frac{1}{2}$; and second, the proportion of the total number of stars in a specified volume of space which have any assigned *absolute* magnitude, M.[1] The latter quantity is

[1] In his discussion Kapteyn defines absolute magnitude as the apparent magnitude which a star would have if it were placed at a distance of 1 parsec from the Earth. Cf. p. 80.

represented by $\phi(M)$, and the mathematical expression for it defines the so-called "luminosity curve." It may, perhaps, be clear that a knowledge of these two quantities is sufficient to make possible a determination of $\Delta(\rho)$—the number of stars per unit volume of space at any distance, ρ.

The accuracy of the determination depends on the accuracy with which the calculated expressions represent the facts. Kapteyn accordingly prefaced his investigation by considering, in collaboration with van Rhijn, to what limiting distances, in various directions, reliable conclusions could be drawn from existing observations. He showed that the luminosity curve could be regarded as having been determined accurately enough to justify its application to any range of absolute magnitude, and therefore to the determination of the star density anywhere in space: the form of the curve was such that extrapolation beyond the results of actual observation was attended by a negligible risk. But the other quantity, N_m, was much more questionable, because reliable counts of stars ceased at a certain apparent magnitude, beyond which the fainter stars were far more numerous than those counted. Kapteyn and van Rhijn considered that, in a very short time, the data would be reliable down to apparent magnitude 17. We can see from Table VI, p. 167, by subtracting successive figures in the second column, that there are more stars of magnitude 18 than there are of magnitude 17, and more of magnitude 19 than of magnitude 18, and so on. Hence, we have not yet reached the magnitude for which the number of stars is a maximum, and extrapolation beyond the well-observed figures is highly dangerous.

A detailed discussion of the limitation thus imposed on our knowledge of the star density in space led to the following conclusions. The data adopted—the luminosity curve and the number of stars of each apparent magnitude appearing in an assigned area of the sky—afford the most extensive plan of the structure of the stellar system at present possible. If we suppose that star counts are reliable down to apparent magnitude 17—which is nearly true, and will probably be quite true in a year or so—and if one or two other very reasonable assumptions be granted, then the star density can be determined throughout the whole region in which it is greater than $\frac{1}{100}$th of its value near the Sun. This region will obviously have a greater extent in the galactic plane than in any other direction, for the star density decreases more

slowly in that direction than in any other. Kapteyn and van Rhijn set the limits at about 8000 parsecs in the galactic plane, and 1700 parsecs in the perpendicular directions. Surfaces drawn through the regions of equal star density were found to be nearly ellipsoidal.

Confining himself within these limits, Kapteyn then made a "first attempt at a general theory of the distribution of masses, forces and velocities in the stellar system." He stated specifically that his object was to obtain general results only, and that quantitative accuracy was considered of secondary importance in comparison with the essential character of the system proposed. Accordingly, he assumed at the beginning that the surfaces of equal star density were actually concentric, similarly situated ellipsoids of revolution, that the

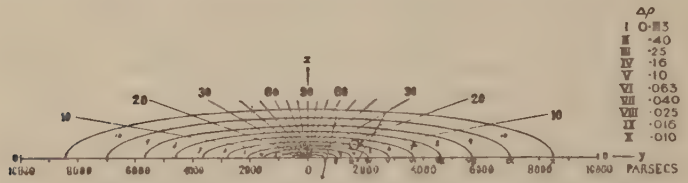

Fig. 30.—Hypothetical Section of Sidereal System (Kapteyn).

axis of revolution was the axis through the galactic poles, and that the Sun was at the centre of the system. Fig. 30 shows a section of the system by a central plane perpendicular to the Galaxy. The star density at the centre being taken as unity, the numbers at the side of the figure show the density at each of the 10 equidensity surfaces whose sections are drawn. No account is taken of local irregularities: only a general view of the system is aimed at. The total number of stars within the region considered is estimated to be about 47 thousand million.

A system of stars in the form of an ellipsoid with an axial ratio of more than five to one, cannot remain at rest: it must be in motion of some kind, and the facts point strongly to a motion of rotation about the shorter axis. Kapteyn assumed that the stellar system had such a motion, and he had to take account also of the random motions of the stars which are actually observed. This flat ellipsoidal system, then, is to be conceived as a congeries of stars, each of which may have its own peculiar motion, but which, as a whole, is

rotating with uniform angular velocity about the shorter axis through the galactic poles. The way is now open for the application of dynamical principles to the system.

Kapteyn here found it necessary to introduce two assumptions in order to make his calculations possible. He assumed, first, that the system was in a steady state—to which there is evidence that it at least closely approximates—and, second, that along the axis, where there is no movement of rotation, the stars could be likened to the molecules of a perfect gas at uniform temperature, so that the laws of the kinetic theory of gases could be applied to them.[1] It was then possible to look upon the column of stars in the direction of the galactic pole as an atmosphere, in which the density decreased with height according to the gaseous laws. Assuming, on the basis of radial velocity measurements, that the average peculiar velocity (*i.e.* the velocity freed from the effects of the solar and stream motions) of the stars was 10.3 kilometres per second, a comparison of the calculated gradient of star density with that represented in Fig. 30 showed that the average mass of a star must decrease with distance from the centre. The actual value of the average mass could be calculated, and Kapteyn found that it must change from 2.2 (the mass of the Sun being the unit), in the second of the ellipsoidal shells drawn in the figure, to 1.4 in the tenth. Such a change is in accordance with more general theoretical expectations.

It appears from this that the Sun is well below the average star in mass. The word "average," however, must be defined more explicitly. As used here, it signifies the total *gravitating* mass within a limited volume, divided by the number of *luminous* stars within the volume : Kapteyn, in fact, to prevent misunderstanding, prefers the phrase, the "average *effective* mass" of the stars. This quantity may not be comparable with the mass of the Sun, for two reasons. First, we know that a large number—perhaps the majority—of the stars are binary, and what is counted as a single star may, in many instances, be two or even more : second, if there is dark matter in the volume considered, it will be included in the total mass, but not in the number of luminous stars, and will therefore increase the effective average. As a matter of fact, the values found by Kapteyn agree very closely with the observed total masses of binary stars, and, taking into account the probability that many apparently single stars

[1] Jeans has pointed out that the second assumption is unnecessary.

are double, he has used the agreement as evidence that the amount of dark matter in the Universe (though, actually, the reasoning applies only to the neighbourhood of the axis of the system) is not excessive.

Turning, now, to the galactic plane itself, the former treatment is not possible without modification, because of the centrifugal forces introduced by the rotation of the system. Kapteyn, however, assumed that the change of average effective mass with star density, which he had found for the stars along the axis, was the same in all directions in the system, and he was then able to calculate the rotational velocities of the stars at different distances. It appeared that, at distances from the axis exceeding 2000 parsecs, the systematic velocity of the stars was nearly constant, the average value being 19.5 kilometres per second. This velocity, arising from the rotation, was, of course, in the plane of the Galaxy.

The striking thing about this result is that there is actually an observed systematic motion of the stars in the plane of the Galaxy—the stream motion. But this involves two opposite directions of motion, and it appears to be rectilinear, with definite vertices, whereas a rotational motion seen from the centre would be circular, with no vertices. Nevertheless, the two motions can easily be identified. In the first place, there is no need to suppose that all the stars are revolving in the same direction. The revolution is necessary to explain centrifugal forces, which would be satisfied with either direction or with both. If, therefore, we suppose that there are two groups of stars, each revolving in its own direction with a velocity of 19.5 kilometres per second, then there must appear from the centre to be two circular streams, with a relative velocity of 39 kilometres per second. The observed relative velocity of the streams is 40 kilometres per second—almost exactly equal to this. To explain why the motion is apparently rectilinear, it must be assumed that the Sun is not at the centre of the system. This is exceedingly probable, on other grounds, and if we suppose that the Sun is at a distance from the centre great in comparison with its distance from the stars whose motions indicate streaming, the difficulty disappears: circular motion observed along small arcs from such a view-point would appear to be rectilinear, and the observed vertices would have no significance except as a clue to the position of the Sun in the system—a problem which we shall take up presently.

Kapteyn's Universe, then, includes star-streaming as a possible, if not an inevitable, phenomenon. The quantitative agreement of the velocities is remarkable, but it is questionable how much importance really belongs to it. Kapteyn, in his general remarks on the theory, lays no stress on numerical results, which must necessarily be only roughly approximate as yet: it would not have been surprising, even assuming the explanation to be correct, if there had been considerable divergence between the calculated and observed figures. It should be remarked also that, according to Kapteyn's view, the velocities of the two streams must necessarily be identical: or, in other words, the standard of rest for the stellar system is given by the average velocity of the stars along the galactic axis. Determinations of the solar motion have hitherto not been confined to this direction, and it would be interesting to see how, if they were so confined, the resulting standard of rest would compare with the standards given by former investigations.

An incidental consequence of Kapteyn's theory, which is of great interest, is the possibility of determining the "velocity of escape" of a star from the system—*i.e.* the velocity which a star must possess in order that, if it encounters no forces other than the gravitational attraction of the system, it may in time travel to an indefinite distance beyond the limits of the stellar universe. Kapteyn finds that, for this to be possible, a star near the centre of the system must have a speed of at least 104 kilometres per second, if it is moving in the direction of the Milky Way, and at least 98 kilometres per second if it is moving in the direction of the galactic poles. The figures are, of course, only approximate, but if we can accept them as they stand, it must follow that a considerable number of stars, particularly of classes *F*, *G* and *R*, will ultimately be lost to the Universe.

Kapteyn's theory, as we have already said, is intended only as a first approximation to the actual arrangement of the sidereal system. It suffers from defects—some, no doubt, unsuspected, and others deliberately tolerated in order to make calculation possible. Kapteyn has not omitted to point out the latter, and has even, in some instances, indicated the directions along which attempts at closer approximations should be made. There is, for instance, the assumption that the equi-density surfaces are concentric ellipsoids. This is not only unproved: it is almost certainly not strictly true.

There is strong evidence that, in the regions near the centre of the system, the equi-density surfaces are spherical, and it is exceedingly unlikely that, in any region, they are truly ellipsoidal. In subsequent improvements of the theory, such an assumption must be dropped.

It is necessary also to determine more exactly the position of the Sun in the system. The appearance of rectilinear star-streaming indicates that it must be at some distance from the centre. Kapteyn has derived an approximate idea of the Sun's position in the following way. Compared with the dimensions of the whole system, the distance of the Sun from the centre cannot be very great, because, as we saw in Chap. XIII., the star density apparently diminishes with distance in all directions, so that the Sun must be in the region of greatest density. But the region of greatest density must be the centre. The Sun can therefore be removed from the centre only to such a distance as would make no perceptible difference in the measurement of star density by the method employed. This distance Kapteyn estimates to be not greater than 700 parsecs, so that we can take this to be the maximum possible value for the distance of the Sun from the centre of the Universe. At the same time, the Sun must be at a certain *minimum* distance, because of the rectilinear appearance of star-streaming. We have seen that it is only in the region more than 2000 parsecs from the axis that the rotational velocity has the constant value agreeing so closely with the stream velocity. In places nearer the axis the disagreement is emphasised, but since the quantitative results are necessarily inexact, we may allow a fair margin of difference without invalidating the theory. Kapteyn finds that, at 600 parsecs from the centre, the rotational velocity would be about 17.5 kilometres per second, and that it would fall off fairly rapidly towards the axis. Taking 35 kilometres per second to be the lowest admissible value of the relative velocity, he regards 600 parsecs as the minimum distance of the Sun from the axis. The actual distance, then, probably lies between 600 and 700 parsecs, and Kapteyn takes, as an approximate value, a distance of 650 parsecs between the Sun and the axis of the stellar system. Furthermore, the fact that the central galactic plane is not a great circle in the sky shows that the Sun is not quite in that plane, and Kapteyn estimates it to be about 38 parsecs to the north.

It remains to determine the position of the centre of the

system as seen from the Sun, and this can readily be done from the observed positions of the stream vertices. If the theory is correct, the line of streaming must be an arc of a circle whose centre is on the galactic axis. The centre of the circle must be in a direction at right angles to the arc, and therefore at a point removed 90° in galactic longitude from the vertices. There are two such points, situated respectively in galactic longitude 77° and 257°. The theory offers no means of deciding between them, but from independent considerations Kapteyn strongly inclines to the former as the true longitude of the centre. Since the Sun is slightly north of the galactic plane, the centre will have a small southerly galactic latitude : Kapteyn finds this to be 3°. The approximate position of the centre is therefore definitely located.

Of the further defects of the theory, Kapteyn specifically notes the uncertainties in the data he employs, which in the future will doubtless be greatly decreased. Still more important, however, is the neglect to take account of the differing characteristics of the various spectral types. As we know, both mass and velocity vary along the Harvard sequence, and stars of different types in the stellar system may be likened to molecules of different elements in the Earth's atmosphere. The chemical composition of the upper air is different from that of the air near the ground, because of the different molecular weights of the elements, and so, in applying the principles of the kinetic theory of gases to the stars along the galactic axis, the results must be inaccurate so long as the differences between the spectral types are ignored. As an example of the way in which the theory might be modified by a discrimination between the types, Kapteyn considers the distribution of the stars having no random velocity at all. Such stars would necessarily lie in the galactic plane, for otherwise the attraction of the stellar system on them would generate a motion different from that of the pure rotational, or stream, motion. Now the Wolf-Rayet stars, which have very small velocities, actually do lie in the galactic plane, and Kapteyn's theory explains this in an extremely simple way, on the assumption that these stars have no random velocity—or, alternatively, random velocities only in the galactic plane. It appears, therefore, that the distribution in space of a class of stars may be very closely connected with its average random velocity.

This theory of Kapteyn's is the crown of a lifetime's devotion to the study of the structure of the Universe. Published only a few weeks before the illustrious author's death, it is the latest word of the school—of which Kapteyn was the acknowledged head and chief—which seeks to build up a system of the stars by reaching from the Sun outwards into space. It makes no claim to be final or comprehensive. In particular, it takes no account of nebulæ or star clusters, and offers no suggestion as to the nature or location of the star clouds of the Milky Way. Whatever opinions may be held with regard to these matters must be of a supplementary character: they can scarcely modify or be responsible in any way to the stellar system as described by Kapteyn.

It is a necessary consequence, however, of the other method of investigating the Universe—the method which works from the outer limits of observation towards the centre—that such features as the Milky Way and globular clusters shall figure prominently in the result, for these objects are the starting points of the inquiry, and define the boundaries of the perceptible universe. Shapley's study of the globular clusters, originally undertaken with the object of learning more about the internal characteristics of these bodies, has led him, step by step, to a theory of the structure of the whole galactic system—a theory which finds a place for almost every type of object which the telescope reveals. Shapley's system, of necessity, differs in almost every particular from Kapteyn's. Shapley emphasises the bodies which Kapteyn ignores. He enumerates the contents of the Universe without connecting them: Kapteyn connects the stars into a single system, and pays no attention to the other bodies. Shapley is more comprehensive than Kapteyn, but less minute in details. He gives a picture of the whole; Kapteyn gives an explanation of a part. It is inevitable that the two systems shall be comparable at some points, for there are regions of space in which they overlap. So far as comparison is possible, the reconciliation appears, as we have said, to be somewhat difficult. It must be left to the future—perhaps the near future—to show how it can be brought about.

We saw in Chap. XVII., that globular clusters, according to Shapley's measurements, occupy a region of space ranging in distance from about 7000 to nearly 70,000 parsecs. In the light of Kapteyn's researches, they would appear, therefore, to be independent of the galactic system altogether—

perhaps other galactic systems of the same kind as ours. But against this conclusion there is the fact that their arrangement with respect to galactic latitude is so symmetrical and of so remarkable a character: they unanimously avoid the Galaxy itself, but crowd towards it in a most striking way. This arrangement would scarcely be possible if they were actually independent systems, and suggests that our plane assemblage of stars extends right into the space between the two sets of clusters in the northern and southern galactic hemispheres.

In support of this view, Shapley has found apparently faint blue stars within the Milky Way, which, on the principles explained in the last chapter, must be at distances of the same order of magnitude as those of the clusters. Moreover, these distances show a large range, so that we cannot suppose that there is an empty space between our system and the region of the clusters: apparently the system stretches continuously out to its farthest limit. For the very distant stars, the ordinary methods of parallax measurement fail, and we are forced to resort to special, and, as we have seen, somewhat dubious methods. These methods, however, have this in their favour, that they give mutually consistent results. The distances of the galactic Cepheids, for example, determined from the period-luminosity relation, are quite in keeping with the hypothesis of a very extensive galactic system.

Supposing, then, that the galactic system occupies the actual space between the two groups of globular clusters, the apparent concentration of the clusters within a semicircle of galactic *longitude* is most naturally explained by assuming that the Sun occupies a very eccentric position in the system, while the clusters are more or less symmetrically situated. The central position of the congregation of clusters would then be in the direction of the centre of the Universe. This direction, pointing to the dense star clouds of the Scorpio-Sagittarius region (galactic longitude 325°), has already been noted in connection with several phenomena—the motion of stars having extremely high velocities, the vertices of the stream motion, the maximum occurrence of novæ, the appearance of quickly moving and of "stellar" planetary nebulæ, the most complete avoidance of the Galaxy by spiral nebulæ. In the light of Shapley's conclusions, the association of a phenomenon with this direction is extremely suggestive. It will be noticed, however, that the centre of

Kapteyn's system has a very different galactic longitude. This is one of the anomalies which have yet to be removed.

Collecting the various data bearing on the dimensions of the Universe, Shapley concludes that the whole perceptible material world forms a single system whose outline is symmetrical about the galactic plane. Apart from the globular clusters and the spiral nebulæ, the cosmic bodies compose a flattened structure, whose extent may be 20 or 30 times as great in the galactic plane as in the perpendicular direction. The greatest diameter of the structure is about 100,000 parsecs.[1] Relatively close to the structure and, in general, approaching it from either side, are the globular clusters. The spiral nebulæ also lie outside the main aggregations but they avoid the region of the globular clusters, and are moving away from, instead of towards, the galactic plane. In this comprehensive Universe the Sun occupies a position about 20,000 parsecs from the centre, and is a little north of the central plane.

The motion of the globular clusters towards the flattened structure—or "equatorial segment," as Shapley calls it—suggests that the segment itself is, at least largely, composed of former globular clusters which, by their mutual gravitational interactions, have become disintegrated. There is no evidence of orderly arrangement in the structure: clusters in various stages of disruption, nebulæ, stars of special types, appear to have been contributed without regard to lateral symmetry, and the semi-independent existence which each unit retains indicates a condition far removed from the ultimate steady state which, in the absence of other influences, gravitational forces must bring about. The appearance of this miscellaneous conglomeration, from the position of the solar system, constitutes the Milky Way: its irregularities of density mark the uneven character of the system. The movements of the planets speak of a cosmos perfectly formed: the Universe as a whole is a chaos—

[1]From a recent study of Star Cloud N.G.C. 6822—a faint structure strongly resembling the Magellanic Clouds—Shapley estimates the distance of this object to be not less than 250,000 parsecs, and possibly 500,000 parsecs. The estimates are based on comparison of angular diameters and brightnesses of parts of the structure with those of similar parts of the Magellanic Clouds and on assumptions with regard to the absolute magnitudes of the brightest stars in the structure. If this distance is correct, the galactic system may be several times greater than the greatest estimate hitherto made, though there appears to be no evidence yet that the cloud in question is not isolated in space.

it shows the reign of law, but certainly not that of order.

Shapley believes that the Universe might be still more extensive than it now appears to be. The amount of dark nebulous matter which it contains is quite unknown. If, as is not improbable, this substance forms, in projection, a more or less continuous band of finite thickness in the direction, say, of the centre of the system, then there will be a wedge-shaped region of space, radiating from the solar system and becoming ever thicker with increasing distance beyond the obscuring material, which is hidden from our sight. It is noteworthy that the most distant of the known globular clusters are farther from the equatorial segment than are those nearer to us—a circumstance which could be explained quite well by the hypothesis of such a band of obscuration.

A system so vast and comprehensive as Shapley's equatorial segment is necessarily beyond detailed investigation at present, and will be so for a long time to come. The stars whose spectra and motion have so far been recorded form but a minute fraction of it. It is evidently of no use to attempt to organise them into a dynamical system along the lines followed by Kapteyn. Not only is the centre of the Universe far outside the boundary of the space they occupy; but the centre itself is apparently only a centre in a geometrical sense. It is not necessarily the centre of gravity, and is certainly not a point of symmetry so far as the movements of the stars are concerned. We must expect to find the Universe in our neighbourhood, as it is in every part of the equatorial segment, a temporary gathering of interpenetrating clusters and groups of stars, permeated with numbers of isolated stars which have wandered away from the groups with which they were originally associated.

Now, superficially at least, this is the kind of thing we actually find. Moving clusters, and the special types of star considered in Chap. XV., seem to be well explained as groups of stars, born in different circumstances, which are now somewhat accidentally assembled in the same region of space. So complex might the assembly be that the analysis into homogeneous systems, even of the stars in our own neighbourhood, might well be a task beyond our powers. Shapley, however, has found strong evidence of the presence of a very extensive local cluster, which must greatly simplify the problem. As a first step in the analysis, he divides the stars,

for some distance round the Sun, into two general classes—those belonging to the local cluster (the *cluster* stars), and those not belonging to the local cluster (the *field* stars). Each class might be, and probably is, capable of considerable subdivision, but, in the first approximation, can be regarded as a single group. The classes are thoroughly intermingled, though quite independent of one another. The Sun is undoubtedly a field star.

There are several distinct reasons for asserting the existence of a local cluster. Charlier, a few years ago, showed that the *B*-type stars—which are the most distant of the stars whose individual parallaxes have been determined—formed a flattened system, lying, as he thought, in the galactic plane and thinning out continuously from the centre outwards. The centre of the system lay in the direction of the constellation Carina, at a distance of about 90 parsecs from the Sun. The greatest diameter of the system was of the order of 1000 parsecs. Charlier was inclined at first to identify the system revealed by the *B* stars with the whole galactic universe: the *B*-type stars, being intrinsically the brightest members, would naturally outline the Universe more conspicuously than would the similarly distributed stars of other types. In Shapley's larger Universe, however, a system of these dimensions must play only a subordinate part—it must form, in fact, a local cluster. This was the first piece of evidence for Shapley's hypothesis.

Affirmatory facts were not slow in arriving. The plane of greatest concentration of the *B* stars is, as a matter of fact, not the plane of the Milky Way at all, but a plane inclined to it at about 12°: the local cluster is evidently not exactly in alignment with the equatorial segment, though it is completely within the boundaries of the segment. It appears to be responsible for a phenomenon pointed out long ago by Gould, that the apparently brightest stars are concentrated along a belt which is not quite coincident with the belt of the Galaxy. Most of these stars are of type *B*, though not all. This concentration has hitherto been looked upon as accidental: it now appears to be a disregarded indication of a second plane of symmetry of the nearer stars.

If Shapley's conclusions are real, it must, clearly, be possible to determine whether a class of stars belongs to the local cluster or not, by finding its plane of concentration. Thus, the *B* stars down to the 6th apparent magnitude

appear to be almost wholly cluster stars—a fortunate circumstance, which made possible Charlier's unconscious discovery of the cluster. The earlier *A*-type stars, however, show a plane of concentration inclined at 5° to the plane of the Milky Way and cutting it along the same line as the plane of the local cluster. This evidently means that the earlier *A*-type stars are partly field and partly cluster stars, and their apparent plane of concentration is therefore the resultant of two actual planes. The stars of the later main spectral types also probably belong to both systems, but their concentration to the plane of symmetry in each system is so much less than that of the earlier type stars that it has not yet been possible to prove this definitely. Diffuse nebulæ, dark and bright, belong to both field and cluster, but mainly to the cluster (cf. p. 313). On the other hand, stars of types *N* and *O*, Cepheid variables, and novæ—all intrinsically very bright stars—are almost exclusively confined to the field. This conclusion throws some light on the problem raised on p. 296.

With the more complete view of the cluster thus obtained, we can fix its position and outline with more precision than was possible to Charlier. Shapley assigns to it a maximum diameter of about 2000 parsecs, and an inclination to the plane of the equatorial segment of from 12° to 15°. Its centre is in the direction of the constellation Carina, or Argo, (galactic longitude, about 250°), and is about 50 parsecs north of the central galactic plane. It is a little less than this distance from the Sun, which is removed less than 10 parsecs north of the central plane of the cluster. The cluster is moving in an almost radial direction from the centre of the equatorial segment in Sagittarius, and its line of motion is apparently parallel to the plane of the Milky Way, and not to its own plane of symmetry. It is much larger than the globular and open clusters, and contains a considerable amount of dark and bright nebulous matter. Shapely thinks it is comparable with the Magellanic Clouds, or some of the star clouds in the Milky Way, rather than with the typical clusters.

The motion of the cluster through the field at once suggests a possible explanation of star-streaming. The simplest hypothesis would be the identification of the cluster and field stars respectively with the two streams, and this was, in fact, the hypothesis first put forward by Shapley. Kapteyn's observation that the overwhelming majority of

B stars belong to stream I. was strongly in support of the assumption. The *B* stars whose motions are observed are almost exclusively cluster stars, and the cluster was therefore identified with stream I. and the field with stream II. It seems likely that the drift of the cluster through the field does lie at the root of the stream motion, but the latter, as it has been observed, is evidently a complex phenomenon,

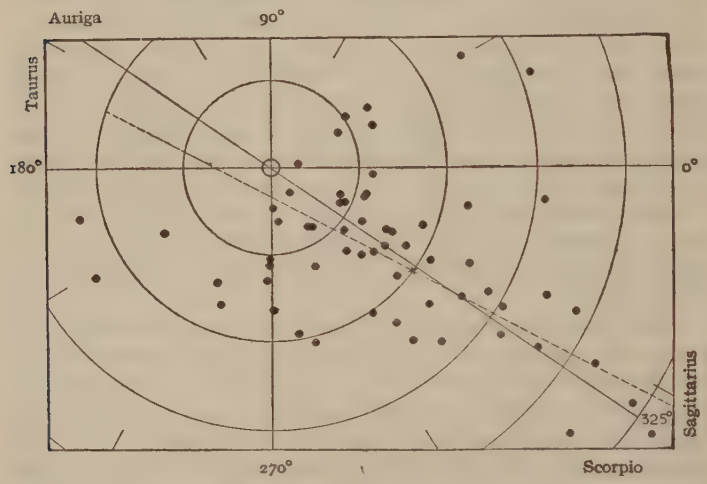

Fig. 31.

The system of globular clusters projected on the plane of the Galaxy. The galactic longitude is indicated for every 30°. The "local system" is completely within the smallest circle, which has a radius of 1000 parsecs. The larger circles, which are also heliocentric, have radii increasing by intervals of 10,000 parsecs. The dotted line indicates the suggested major axis of the system, and the cross the adopted centre. The dots are about five times the actual diameters of the clusters on this scale. Nine clusters more distant from the plane than 15,000 parsecs are not included in the diagram.

involving more than a single type of motion. Thus, Stromberg's detection of the identity between the streaming of stars of certain spectral types and the motions of moving clusters shows that the matter is not to be explained in a very simple way. Shapley inclines to the view that some, at least, of the moving clusters are not independent systems, but are subordinate parts of the local cluster, which has

somewhat intricate internal movements. The field also can hardly be homogeneous. The observed streams are probably the resultant effect of the motion of the cluster through the field, and of internal movements, particularly in the former, which, operating in the plane of the larger Galaxy, are not easily separated from the drift of the cluster as a whole. It seems clear that, when the analysis of the region round the Sun enters on its second stage—the investigation of the internal structure of the cluster and the field—the movements of the stars will play a leading part.

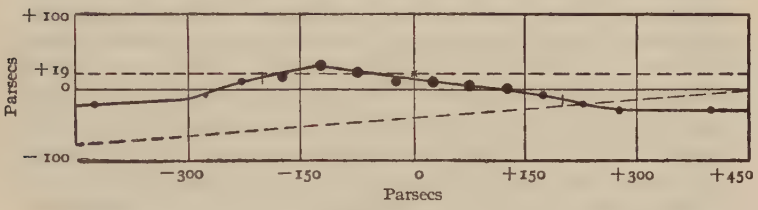

Fig. 32.

Projection of the local cluster of B stars on a plane perpendicular to the Galaxy and to the projection, on Charlier's adopted plane, of the line joining the Sun and the centre of the cluster. The centre of the system of B stars, as derived by Charlier, is at the origin of co-ordinates. The cross shows the position of the Sun. The true galactic plane is indicated by the inclined broken line.

The fact that the Sun is close to the centre of the local cluster must be at least partly responsible for the decreasing star density with distance in all outward directions. If the Earth is inhabited in a few million years' time, when the cluster has passed on, the astronomers will, in all probability, find no such delusive evidence that the Sun is at the centre of the Universe. If this is the true explanation, then Kapteyn's sidereal system should correspond with the local cluster rather than with the equatorial segment, though the correspondence must be far from complete because of Kapteyn's inclusion of the field stars in his counts. And, apart from this, there is one reason why the local cluster can account for only a small part of the observed distribution of stars. Its greatest extent, according to Shapley, is 2000 parsecs, whereas Kapteyn's system extends for at least 17,000 parsecs, with a continuous and almost uniform radial falling off of

density in all directions. If, therefore, the local cluster gives rise to the falling off in the immediate neighbourhood of the Sun, we must suppose that, at the greater distances, the field shows a similar thinning out. Evidently the settlement of the matter awaits the analysis of the structure of the field, or a conclusive determination of the actual dimensions of the Universe.

It is clear, however, that the principles on which Kapteyn fixes the centre of the sidereal system have no meaning from Shapley's point of view. Kapteyn's explanation of star streaming forces him to place the centre at 90° from the stream vertices. Shapley's explanation arises from the observation that the centre of the Universe is in the *same* direction as one of the vertices. Nor is there any correspondence between Shapley's derivation of the centre of the local cluster and Kapteyn's derivation of the centre of his system : the one depends primarily on the measured parallaxes of *B* stars, and the other on a particular hypothesis of star-streaming which, from Shapley's point of view, is inadmissible. There is no sign, at present, of a blending of the two theories.

Shapley has illustrated his view of the Universe by a number of diagrams, of which three, by his kindness, are reproduced in Figs. 31, 32 and 33. A comparison with Fig. 30 shows very well the difference, not only in the conclusions, but in the general character of Shapley's and Kapteyn's systems. The outstanding feature of the one is comprehensiveness, and that of the other, thoroughness. With regard to the reliability of Shapley's results, it should be said that criticism has been directed against the scale of his system rather than against its form. The distances of the globular clusters, the Milky Way clouds, and the Cepheid variables are determined on the same or consistent principles, and seem to be at least relatively accurate. So far as can be seen, the only reason for doubting this is that there might conceivably be absorption of light in space which does not involve scattering of the shorter wave-lengths, and that the cause of the absorption is not uniformly distributed in all directions. We say " conceivably," because absorption of this kind (other than absorption by opaque bodies) is purely hypothetical. If it should exist throughout space, then, as Eddington has pointed out, the whole scale of Shapley's system will have to be reduced. But, until we have definite

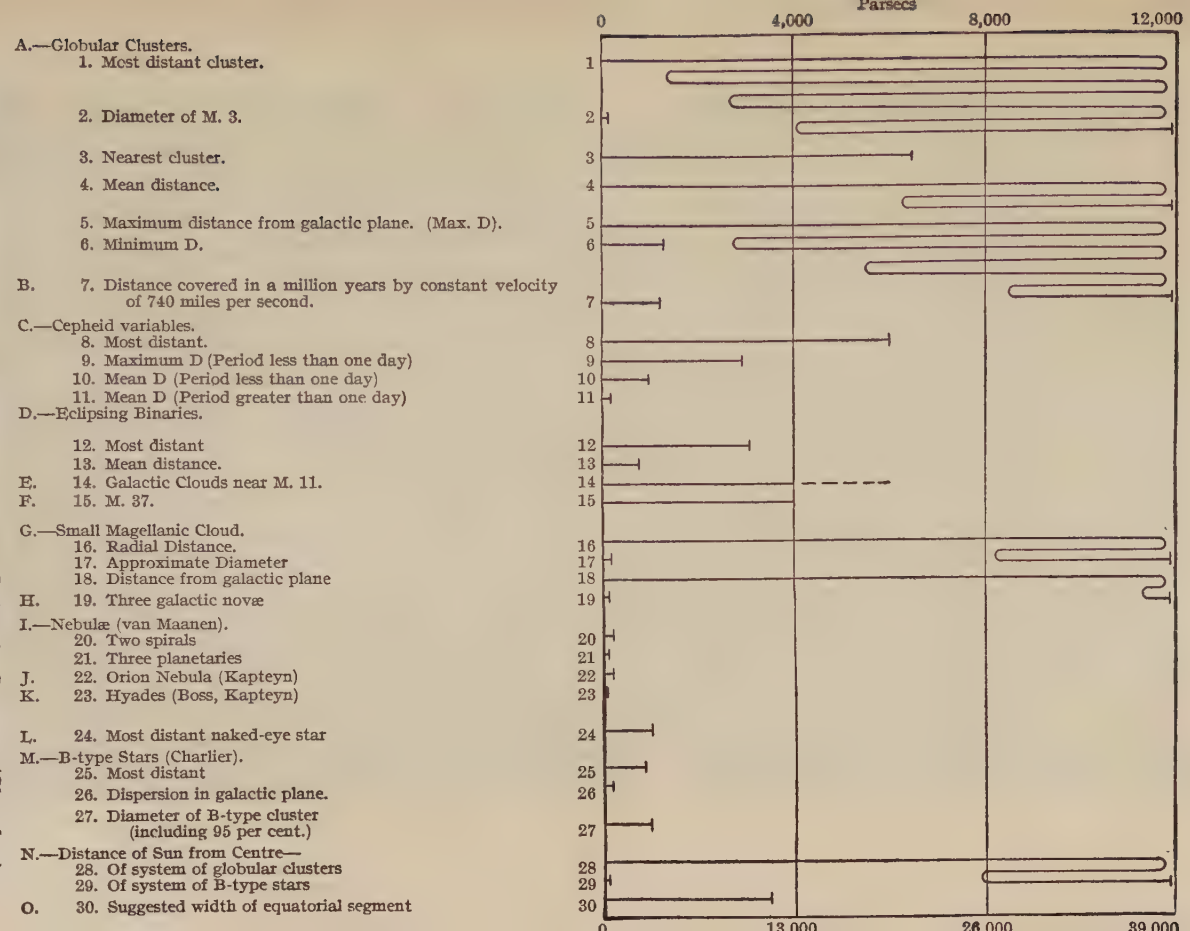

Fig. 33—Comparison of Distances in the Galactic System (Shapley).

evidence of a phenomenon of this kind, there is probably little justification for assuming its existence.

Up to the present, Shapley has not indicated very precisely the positions of the spiral nebulæ in his plan of the Universe. He has pointed out, however, that, in the matter of galactic longitude, they appear to avoid the globular clusters—a conclusion which has recently been confirmed by Reynolds. Reynolds suggests that the relative distances of the spirals might be ascertained on the principle applied by Shapley to the globular clusters—that the actual diameters of similar bodies are of the same order of magnitude. A reliable parallax of one of the spirals would then make known the parallaxes of all. If the method is sound, it is clear that the spirals must have an enormous range of distance, and must extend far outside the equatorial segment. Such a possibility gives an additional interest to the question of their nature, and, in particular, to the island universe hypothesis which was described in Chap. XVI.

There is no doubt that Shapley's theory places the island universe hypothesis is a very difficult position. If our Universe has actually the dimensions assigned to it by Shapley, and the spirals are at all comparable with it in size, they must be at enormous distances—far greater than the tentative parallax measures imply. Moreover, with the disappearance of the Milky Way as a separate entity, the significance of the spiral arms of the nebulæ disappears also. Even if Shapley's scale is wrong, the universe he pictures is not easily conceived as having a spiral form. Nevertheless, it is not certain that the spirals are not other universes, even if Shapley's views of our own system are correct, though it seems that, in that event, our Universe must be exceptionally large. It will be appropriate now to consider more completely than was possible in Chap. XVI., the arguments for and against the island universe hypothesis.

The great strength of the hypothesis lies in the straightforward and simple way in which, as we have shown, it explains most of the observed characteristics of the nebulæ—particularly their remarkable distribution in galactic latitude. But that is not all. Curtis has made out a very strong case which includes several other points. The theory avoids the necessity of finding a place for the spirals in the evolutionary scheme. If these bodies belong to our own system, we must either find some "blood-relationship" between them and

the stars, or else admit that they belong to a separate line of development of the stuff of which they and the stars are made. Each of these alternatives presents difficulties which are not readily overcome.

Again, if Kapteyn's view of the Universe be a true one, and the Milky Way (as, on independent grounds, has been proposed by Easton) has a spiral structure and surrounds the flattened system of stars, then there are strong grounds for supposing that the spiral nebulæ have both the form and the dimensions of our Universe, and therefore are most reasonably interpreted as other universes of a similar kind. Such a view is held to be consistent also with the phenomena of the novæ which have been observed in the brighter spirals. Assuming the novæ to be essentially similar in character to those of our own Galaxy, the distances calculated from their apparent magnitudes denote dimensions for the nebulæ of the same order as those adopted for our own galactic system before Shapley's researches were made. The objection stated on p. 331, however, is still applicable.

On the other hand, the characteristics of the spirals can be explained fairly well on the assumption that the bodies are gaseous or nebulous. This hypothesis does not account for the space distribution, and possibly needs a further assumption to explain the high velocities, but it has the advantage over the island universe theory with respect to the occurrence of the novæ and to internal motions and the parallaxes and masses calculated from them. It has also the great merit of fitting in equally well with both Shapley's and Kapteyn's sidereal systems. Shapley meets the difficulty of fitting the spirals into an evolutionary scheme by suggesting that they represent an evolution not directly connected with that of the stars. This, however, is by no means a necessary conclusion. It is possible, as we shall see in the next chapter, to connect the spirals directly with the stars, and thereby to assign to them a place in the Universe as important as that which they are given by the island universe hypothesis.

The almost universal recession of the spirals needs a special assumption to explain it, whatever view is taken of the character of the bodies. Shapley supposes that there is some repulsive force at work between the equatorial segment and the spirals. In the segment itself, gravitation is the prevailing force, but apparently between the segment and the

spirals this is either non-existent or else is overcome by some unknown force. If the spirals actually represent a different order of evolution from that of the stars, we are, of course, under no necessity to suppose that they submit to gravitation. On the other hand, the approach of the Andromeda nebula, which is probably the nearest of all the spiral nebulæ, may indicate that gravitation is at work and is overcome by the unknown repulsive force only at great distances. If the island universe hypothesis be adopted, then we are bound to suppose gravitation to be at work, and the repulsive force must act, not only between the galactic system and the spirals, but among the spirals themselves. The approach of the Andromeda nebula may then be due to gravitation overcoming the repulsive force owing to the relative nearness of the nebula, or else to the resultant effect of all the attractions and repulsions between the various universes, including our own.

The idea of a repulsive force, however, is, according to Russell, inadmissible, because such a force would deform the nebulæ. Russell suggests two alternatives, which appear to exhaust the possibilities of accounting for the recession of the spirals. According to the first, the spirals and our own Galaxy are to be looked upon as parts of a far more comprehensive gravitating system, of which we have observed but a very little. The motions of all the cosmic bodies would then obey the larger system, and would have to be accepted at present as fundamental data rather than as facts to be explained. The second alternative is that the phenomenon might arise from the properties of space, according to the theory of relativity : if so, the displacements of the spectrum lines would not be wholly due to velocity. We have previously referred to the possible displacement towards the red of lines originating in a region of high gravitational potential. There is yet another possible effect of relativity, to which de Sitter has called attention ; namely, a similar displacement of lines arising in very distant objects. If this is the true explanation, the distances of the spirals must be much greater than those of the globular clusters, because the spectrum lines of the latter are, as a rule, displaced towards the violet ; *i.e.* the clusters appear to be approaching the Galaxy. The island universe hypothesis would, in that event, be tenable, even if Shapley's scale of the Universe were substantiated.

It will be realised that the crux of the spiral nebula problem

lies in the question of the dimensions of the galactic system and the distances of the spirals. Other data add weight to the respective sides, but, so far as we can see, without an unquestionable determination of these numerical quantities the issue will remain in doubt. Until the conflicting evidence can be reduced to unanimity, there will probably be advocates of both views.

CHAPTER XIX

AS IT WAS

The study of cosmic history is the most difficult, the most doubtful, and the most hopeful of all attempts to look into the past. Human history—regarded for comparison merely as a record of actual events—can draw on the written evidence of eye-witnesses. Intelligent beings, who lived and took their part in the happenings of the past, have left intelligible accounts of the world in which they lived—accounts written generally in language whose interpretation admits of no question. Geological history also has its records—not preserved of set purpose, perhaps, but none the less reliable. It has, moreover, the inestimable advantage that the forces whose combined effects have moulded the Earth into its present form can be watched, even now, working their changes in the globe we inhabit. The Earth on which we are born is not the Earth we leave at death. But cosmic history has nothing but the present. Every avenue back into the past shows a Universe sensibly identical with the Universe of to-day. There is no record of a different world : there is no perceptible change in the representative cosmic bodies to show us the lines along which they have developed. The problem of cosmic history is the problem of drawing an infinite curve, having given but a single point. Nevertheless, the workings of Nature in the Universe as a whole are comparatively so simple and direct that there is a brighter prospect of unravelling them than there is of solving the smaller but more intricate problems of human and geological development. The data are fewer and less definite, but they are more amenable to the processes of reasoning.

Let us see what we mean when we say that no perceptible change has taken place in the heavenly bodies in recorded times. The geological record extends backwards for millions of years, and its character is such that there can have been but little, if any, continuous change in the temperature of the Earth's crust from the remotest periods up to the present. The Earth's temperature during the whole of that time must

have been determined primarily by the heat received from the Sun, and this, in turn, must have depended almost entirely on the temperature of the Sun. We conclude, therefore, that the Sun has maintained sensibly the same temperature for millions of years. And since, if Russell's theory be true, the temperature is one of the fundamental characteristics of a star and is correlated with almost all of the stellar qualities, it must follow that the Sun has remained almost completely in the same state during that enormous length of time: it was, in all probability, a dwarf star of *G* type when life began on the Earth.

It is true, of course (see p. 174), that the Sun may now be at a stage in its development at which its characteristics remain constant over the longest period, but there are other observations which suggest extreme slowness of stellar change. We cannot look back for such long periods in the lives of other stars, but, so far as we are able to go, the evidence is precisely the same: there is no sign of even the smallest change. If Shapley's scale of the galactic system be correct, our present view of the Universe is a panorama extending continuously back through 200,000 years. If, in that time, there had been a considerable development of stellar properties, there would be a continuous change, with distance and time, in the relative numbers of stars of different spectral types—partly obscured, perhaps, by differences in the dates of birth of the various clusters. No such change is discernible. Among the giant stars—which are all that can be observed in the most distant systems—the distribution of the spectral types is practically the same in the near and the distant clusters. Even in the open clusters—which, on Shapley's hypothesis, must have taken millions of years to reach their present state from the original globular systems, we find giant stars in an early stage of evolution. Cepheid variables tell the same tale: the apparently absolute constancy of their mean periods points to an evolution, if at all, of inconceivable slowness.

And yet there must be change. How otherwise can we account for the Harvard sequence and the consistency of Russell's theory of evolution? Moreover, we know that a star is pouring out energy into space at a tremendous rate. To take the Sun again as an example, the amount of energy received by the Earth indicates that the Sun is radiating at the rate of about 0.6×10^{24} horse-power.

The radiant energy which it receives from the rest of the Universe can be estimated, and is found to be practically negligible. Unless the principle of conservation of energy is a delusion, therefore, the Sun must be losing energy continuously at the rate mentioned above. It is impossible to assume that this can go on without a change of some kind taking place in the condition of the Sun. If the Sun has not an inexhaustible store of energy, it must, sooner or later, show signs of suffering from its unstinting prodigality, and what is true of the Sun is true, in essence, of the rest of the stars. Theory and observation alike demand the assumption of an evolving Universe.

We have, then, no alternative to the supposition that the stars were once different from what they are now, but at what rate they have changed we do not know. Two conclusions seem to be fairly well established : first, that the more massive a star is, the slower is its development ; and, second, that a given star passes through the giant stage of its career more quickly than it passes through the dwarf stage. If, therefore, we knew the masses of all the stars, we could ascertain fairly well their relative conditions at any previous time in the world's history—at least back to the giant *M* stage of each star. We cannot, of course, discover when novæ have blazed up, nor even what stars in our present firmament have in the past appeared as novæ : there is reason to believe that their number is much larger than, until recently, was generally assumed. But we must still regard these bodies as exceptions to the normal results of natural processes, and, for the majority of the stars at any rate, we feel fairly confident in supposing a course of evolution essentially similar to that described by Russell.

Looking backwards in time, then, we may draw one or two broad conclusions about the changes that have taken place. It appears that there has been a gradual diminution of the volume of space occupied by matter—perhaps ever since the hypothetical moment when the world began, but, at any rate, ever since the material of the world was in large measure formed into stars. Again, there has been a gradual diminution also in the total amount of energy radiated into space in a given time. These conclusions follow at once from Russell's theory, because contraction and loss of radiating power (measured partially by decrease of absolute brightness), continue throughout stellar history. The total mass of the

Universe we can perhaps safely regard as unchanged, though, in our ignorance of all that might take place in the interiors of stars, and especially in view of the possibility that matter, as we know it, may have been formed by a gradual process, it is well not to be dogmatic on this point. There appears, however, to have been taking place a process of differentiation of matter into more and more bodies. As we go back, the components of multiple stars approach one another and at length coalesce; satellites unite with their primaries, and planets with their suns. In the journey backwards through time, we find a tendency towards unification, as well as towards expansion and increased radiation.

These conclusions are of the most general character, and are liable to mislead us altogether unless we take into account another factor—the motions of the stars. The simple picture of a Universe which appears, on looking back into its youth, to become reduced to fewer bodies, radiating more copiously and occupying more and more of the space allotted to them, is almost certainly not true to fact. The double stars we see may once have been single stars, but who can say how many stars which once belonged to our system have now left it, or how many have come into it from the outside? The stars may once have been larger and brighter, but interstellar space need not therefore have been less extensive or more densely filled with radiant energy: the stars may have been much farther apart than they are now. Until we know how the stars have been moving, we can say nothing at all on these points.

We can assure ourselves, too, that the effect of stellar movements on the form of the Universe is by no means necessarily negligible. Consider a star moving at the rate of 18.6 miles per second—not very different from the average speed of a star. This is $\frac{1}{10000}$th of the speed of light. Now we have seen that bodies at the extremity of the visible universe are in practically the same state of evolution as are those in our own neighbourhood. During the time the light of these bodies has been travelling to us, our star will therefore have moved through a distance equal to $\frac{1}{10000}$th of their distance from us. While it has been moving through this distance, there has therefore been no perceptible change in the physical state of the cosmic bodies. Now there are a large number of stars moving more quickly than this—to say nothing of the spiral nebulæ and globular clusters—and we

must therefore assume that if we go back through, say, the average time occupied by a change from one spectral type to the next, a star must have moved through at least a considerable fraction of the extent of the galactic system, and the form of that system may have entirely changed. This argument is quite independent of any assumption as to the actual scale of the Universe.

It is evident that we are face to face with a double problem. There is, first, the former state of each individual body to be considered; and, second, the changes in the structure and general form of the Universe as a whole. It may be that these problems are not distinct: the evolution of a body may be determined to an important extent by the general configuration of the Universe. Jeans's theory of the formation of spiral nebulæ, for instance, attributes the shape of these bodies entirely to external influences. It is a matter of observation, however, that, at the present time, the stars in general influence one another only to a negligible degree, and we shall assume, at least as a working hypothesis, that the two problems are independent. We will consider them, therefore, in turn, dealing, as far as possible in a limited space and a state of profound ignorance, with the difficulties they present.

A typical dwarf star, of average mass, can be traced back almost completely through its stellar life. Reversing the process of its development, we find it getting hotter and larger up to a certain point—say, the *A* condition—then turning round and retracing its passage through the spectral types, passing from *A* to *F*, from *F* to *G*, getting gradually cooler but larger and more luminous, until it reaches the giant stage of type *M*: and then? One thing seems certain, and that is that before becoming a giant *M* star, a mass of material cannot be a luminous star at all: it must exist in some other form. We cannot escape this conclusion because we find large numbers of giant *M* stars in the sky, but, so far as we know, not a single star which can be regarded as immediately preceding them in type. Unless, therefore, the process of creation of stars—which, from the evidence of the existing spectral types, has probably been going on continuously from the birth of the oldest to that of the youngest star—has suddenly ceased, the giant *M* stage must have been preceded by a non-stellar condition.

We know of but one set of non-stellar bodies that can

conceivably develop into giant stars—the nebulæ. It may, of course, be that the precursors of the stellar state are dark bodies which have not been observed at all: certainly bodies just about to become stars would scarcely be discernible without their luminosity. It is, therefore, probably useless to look for a continuity of development before the stellar state has been reached. But if we assume that consolidation is a continuous process from the birth of matter up to its present state, we may reasonably look for evidence of a condition long preceding that of a star, in which the material has sufficient angular extent to be discernible even though it does not radiate. Matter in this form appears to exist in the nebulæ, and it is therefore the general belief of astronomers that nebulæ and stars represent more or less widely separated events in the history of the same substance.

But there are different kinds of nebulæ, some at least of which appear to have no genetic connection with one another. We must consider which of them fulfils most satisfactorily the conditions necessary for development into stars. At first sight, of course, the planetary nebulæ appear to be just the objects we are looking for. They have central stars—in some instances, central condensations of finite apparent size—which might be regarded as the partial development of the nebulous matter into the earliest stellar stage. If the telescope and the camera were the only astronomical instruments, it is difficult to see how a conclusion of this kind could be avoided. But in these days of the spectroscope, it is impossible to maintain this hypothesis. The spectral type and the character of the continuous spectra of the nuclei of planetaries, to say nothing of their other characteristics, are conclusive evidence against it. Moreover, planetary nebulæ are too scarce, in comparison with the numbers of the stars, to admit of the idea. We are forced to regard them, together with novæ and some, at least, of the Wolf-Rayet stars, as belonging to an exceptional order of evolution.

A much stronger case can be made out for the diffuse nebulæ. Since it has been realised that they shine, in all probability, by borrowed light, there is no characteristic in which they appear unfitted for the rôle of star generators. They lie where the stars are most thickly crowded; their densities are very low, and they are probably cold; they consist—if the hypothesis that they are dust and gas be true—of both solid and gaseous material, and therefore of different

chemical elements, ready to be vaporised completely as their temperatures rise ; and they appear to be sufficiently abundant to supply stars comparable in number with those actually found. We do not find them in association with giant *M* stars—there are very few such stars that are known to be involved in nebulosity—but since there is, by hypothesis, a considerable gap between the latest observed nebular and earliest observed stellar stages, and since the velocities of the *M* stars are large, there is no reason why there should be such an association. Allowing for the hidden intervals between the nebulæ and the stars, the characteristics of the two kinds of body accord with one another as closely as could be expected.

With regard to this interval, however, it might well be argued that it should not be quite so wide as it is. If, as we believe, a single nebula contains sufficient material to form many stars, there must come a time when it breaks up into the individual stars or their progenitors. According to Eddington's theory, the breaking-up is a consequence of instability arising from the intensity of radiation pressure within the mass. Now radiation pressure cannot reach the critical value unless the material is very hot, and therefore presumably visible. We ought therefore to find bodies, many times more massive than the average star, in the act of disruption. Perhaps we cannot say definitely that we do not find such bodies. Most of the very youngest stars are variable, and it may be that the variation is the outward and visible sign of the instability which is to prove their undoing. Unfortunately, we have no means of measuring the masses of these stars.

In a recent paper, Russell declared that, as the parents of stars, " all classes of nebulæ except the extended gaseous " (*i.e.* diffuse) " nebulæ have already been excluded from consideration," and he apparently looked with no great favour on the case for the diffuse nebulæ. Jeans, however, has given very cogent reasons for supposing that the *spiral* nebulæ are the bodies which ultimately develop into stars, and he has shown also how the development might take place. We know that the arms of these bodies appear as successions of condensations, and that, on Jeans's hypothesis, it is possible, for certain of the nebulæ, to calculate the mean distance between adjacent condensations. If the mean density of the matter in the arms were known, it would be possible to determine also the average mass of the condensations. We

do not exactly know the mean density in the arms of any of the spirals, but it can be estimated within fairly narrow limits when the density of the nucleus is known—as in *M*33, for example (p. 335). The resulting average mass of a condensation, for each of the nebulæ examined, turns out to be about the same—a quantity of the order of 10^{34} grams, which is approximately the average mass of a star. Jeans concludes, therefore, that the condensations in the arms of spiral nebulæ are incipient stars.

It should be observed that the average mass of a condensation is determined almost entirely by the density of the nucleus of the nebula, and this, in turn, by the period of rotation. It has nothing whatever to do with the total mass of the nebula. Thus, if one nebula is 100 times as massive, but has the same rotation period, as another, the condensations in its arms will not be 100 times as massive, but there will be 100 times as many of them, having the same mass. Here, then, is a possible explanation, totally different from Eddington's, of the approximate equality of stellar masses: it is a simple consequence of the approximate equality of the periods of rotation of the parent nebulæ. Why these periods should be so nearly equal, and why, being equal, they should be what they are, or have been, are questions which cannot yet be answered.

If this theory be correct, we are able to follow a star at least from the elongated nebular state up to the time of its separate existence, and it may even be possible to go farther back still—to the globular and irregular nebulæ. We may suppose that the irregular nebulæ, by their rotation, pass into the globular and elongated forms, and thence into the spirals, which shed the stars, one by one, as condensations moving outwards from the central nucleus. The process continues until the nebula has been completely broken up into bodies of stellar mass, which compose, when the bodies are mature stars, a globular cluster. Soon after its birth, it appears that a star—which is then in an extremely rarefied condition—cools by radiation and becomes dark: we have seen already that this assumption must be made in order to reconcile the probable age of the spirals with the observed velocities along the arms. From the time of darkening until the time when it appears as an *M*-type giant, the course of events is left to our imagination. There is, however, no difficulty in picturing what happens. The star begins to contract by

virtue of its own gravitation, and in so doing grows hotter and hotter until ultimately it appears as a luminous body. The tale of evolution is complete from the irregular nebulæ to the latest dwarf stars.

This is the most comprehensive scheme of material development that has yet been proposed. It finds a place for almost every type of cosmic body that has been detected. Planetary nebulæ, with novæ and Wolf-Rayet stars, stand outside, as would be expected. The only other objects that are not included in the scheme are the diffuse nebulæ, and their omission is much more serious. It is conceivable that they come at the very beginning, before the non-galactic nebulæ, but there is very little, either in the distribution or in the characteristics of the bodies, to support this suggestion. Or they might be the accumulated by-products of stars—material gradually lost to stars by the escape of molecules from the atmospheres or by electrical discharge, as the corona is conceived to be discharged from the Sun. Or, again, they might be material of the kind which, outside the Galaxy, in a state of relative freedom, can develop into the nebulæ of regular shape and finally into spirals and stars, but which, entangled in the galactic system with its various forces—gravitation, radiation pressure, and the rest—is distorted and given only turbulent motion instead of rotation. The last-named explanation would account at once for the mutual exclusiveness of galactic and non-galactic nebulæ, and the total absence of spirals from the galactic plane. But the matter is shrouded in obscurity at present, and no definite conclusion can be reached.

Another difficulty, which is common to all attempts to continue Russell's theory of evolution backwards from the giant *M* stage, is the explanation of the apparently sudden development of great luminosity when a star begins to be visible. The giant *M* stars are among the brightest stars in the sky, and yet we are to regard them as having but lately emerged from a state of total darkness. One of Russell's arguments, when his theory was fighting for existence against the older theory of evolution, was that, according to the older theory, we must suppose a brilliant star like Betelgeuse to be dying of old age—a supposition which could scarcely inspire confidence. It may, however, superficially, at least, be argued with equal justification that it is no less difficult to suppose such a star to have just been born. Indeed, if analogy with other natural processes is permissible, the latter

supposition is the less plausible: human beings frequently die in the pride of manhood, but what instance is there of any having been so born? Evidently an explanation of some kind is called for.

So far as we can see at present, it is not possible to give an explanation in terms of known processes. It is indeed surprising, when we consider how much wider is the range of physical processes in the stars than in our laboratories, that we should have been able to investigate so closely as we have done the conditions obtaining in the interiors and atmospheres of stars. Russell has proposed that there is, in a star, an "unknown source of energy," which is released under certain conditions that we have not yet reproduced, and which, coming suddenly into play, gives a star the great luminosity with which it begins life. This hypothesis has also in its favour the fact that it can account for another remarkable phenomenon that is otherwise apparently inexplicable—namely, the extreme slowness with which the evolutionary changes of a star take place in spite of the continuous radiation of energy into space. It was thought at one time that the heat produced by the contraction of a star was sufficient to account for the radiation, but this idea can no longer be held in face of the recently acquired evidence regarding the duration of a star's life. There must be another source of energy, and Russell's hypothesis may be extended to meet the need.

If it is so extended, the facts of observation provide us with a fair amount of knowledge of the conditions under which the unknown source is tapped, and of the manner in which it yields its supply. We must suppose, first of all, that it comes into play suddenly when the contracting embryonic star has become hot enough, by contraction, to release it. Secondly, it must be almost, but not quite, sufficient to balance the energy radiated into space, and since the rate of radiation varies a great deal during a star's career, the rate at which energy is supplied by the unknown source must be supposed to vary also: it must, in fact, depend in some way on the demand. With these conditions as a guide, Russell has formulated the following imaginary course of development of a star.

In the period immediately preceding the giant *M* stage, the star is contracting and thereby getting hotter. When the interior reaches a certain temperature, something happens

which calls forth the "unknown" supply of energy in a comparatively small region round the centre of the star where the temperature is highest. The energy thus made available is exceedingly great, but finite, in amount, and the rate at which it is discharged depends both on the temperature and on the amount of it which remains: an increase of temperature tends to accelerate the supply, but the gradual exhaustion tends independently to retard it. Before the star has reached the giant *M* stage it contracts rather rapidly, and consequently rises rapidly in temperature. But as soon as the unknown source begins to work the contraction is checked, for a heat wave spreads out from the centre and tends to expand the surrounding layers. A state almost of equilibrium now sets in, for, if the star were to contract, the heat thereby generated would call forth an increased supply of energy from the unknown source which would check the contraction; and if the star were to expand, it would necessarily be cooled, the unknown supply of energy would come more slowly, and the expansion would cease.

The equilibrium, however, is not perfect, because of the gradual falling off of the unknown supply as it gets exhausted. As a star progresses through the giant period of its life, it makes a constant demand for energy to keep up its almost uniform rate of radiation. Since the supply in the initial central region is being discharged more slowly, the star must contract. The interior of the star thus rises continuously in temperature, and so a greater and greater volume attains the condition in which the unknown supply is released. We have therefore a very slow process of contraction and heating. There will obviously be a limit to this process when the supply has become so depleted that the star can no longer call upon sufficient energy to maintain its radiation undiminished. It will be forced to radiate at a smaller rate: contraction will continue, but the temperature will fall, because the heat generated by the contraction cannot induce the impoverished unknown source to maintain the temperature in spite of the radiation. This is the condition of a star in the dwarf stage. When the star passes out of existence as a luminous body, the unknown source must be almost, if not quite, exhausted.

The nature of the unknown source is, perhaps, not a matter on which it is profitable at present to speculate, though it is difficult to avoid doing so. We know of only one receptacle of energy which has resisted all attempts to unlock

it, and that is the nuclei of atoms. Spectroscopic and chemical phenomena are associated with the surrounding electrons: the nuclei, so far as we know, are undisturbed by terrestrial events, except in the special processes of radio-activity, and the enormous stores of energy which are therein released indicate something of the possibilities which might result from the power to control atomic nuclei generally. The very stability of the so-called "permanent" elements, however, shows that energy is to be expected from their building up rather than from their breaking down. We have to supply energy to disintegrate them—more than we have yet been able to command—and if we suppose that they were once formed from simpler constituents, we must suppose also that an equivalent amount of energy was then released. Eddington has suggested that the process of formation of atomic nuclei takes place in the stars, or in the parents of stars, and may be identified with Russell's "unknown source of energy."

There seems to be little doubt that the energy set free in the supposed formation of atoms would be sufficient to account for the radiation from stars throughout periods consistent with all our observations. The nucleus of every atom can be regarded, without doing violence to any fact or theory of physics, as a complex of hydrogen nuclei and electrons. The masses of all nuclei are very nearly integral multiples of the mass of the hydrogen nucleus—but not quite. It is in the small deviation from the exactness of this relation that the sufficiency of Eddington's supposition becomes manifest. As a particular example, the helium nucleus can be looked upon as a system comprising four hydrogen nuclei and two electrons, the relative mass of the latter being negligible. But if the mass of the helium nucleus is represented by the number 4, that of the hydrogen nucleus is not 1, but 1.008. If, then, helium has been formed from hydrogen, there has been a loss of mass of $4 \times 0.008 = 0.032$ units. Mass, we believe, does not "pass into nothingness," and the "loss" must therefore be accounted for. According to the hypothesis it becomes energy. In the modern electro-magnetic theory mass and energy can be expressed in the same terms, so that the transformation is theoretically possible, and the amount of energy corresponding to a given mass can be determined. Eddington calculates that "if 5 per cent. of a star's mass consists initially of hydrogen atoms which are gradually

being combined to form more complex elements, the total heat liberated will more than suffice for our demands, and we need look no farther for the source of a star's energy."

It is not easy to see how this hypothesis can be tested: the time is scarcely ripe as yet for it to be placed on a firm basis. It implies that, on the whole, the atoms in a dwarf star have a greater average atomic weight than the atoms in a giant star, but a change of 5 per cent. of the mass, occurring at the centre, is impossible to detect. If, however, the hypothesis be extended, and the change is regarded, not merely as an accidental occurrence called forth by certain transient circumstances in the history of stellar material but as the sole original process by which the more complex atoms are formed—then the balance of existing evidence seems to be strongly against it. We do not know with certainty what a nebula is. If we assume the diffuse nebulæ to be the ancestors of stars, and interpret the spectroscopic evidence as indicating that they contain only hydrogen, helium, and "nebulium," then we have, it is true, a starting point consistent with the hypothesis: we have the original matter consisting only of the simplest elements. But the earliest stars, the *M* giants, tell quite a different story. Titanium oxide and the metals are the prominent substances in their atmospheres, and these are anything but simple: we have highly organised molecules in abundance in the coolest parts of the stars in which the process which is conceived to form them has lately begun. Indeed, the spectroscopic evidence is not easily explained, even on Eddington's restricted supposition. Hydrogen is relatively inconspicuous in the atmospheres of *M* giants (unless it exists as molecules, in which condition it does not appear to have been sought), and it is hard to suppose that around the centre of the star, where the heaviest materials would be expected to congregate, it is to be found to the extent of 5 per cent. of the whole stellar mass. The sequence of spectral types could be used much more plausibly to support the converse hypothesis—that, at the highest temperatures, the complex elements are changed into simple ones: it is in the *A* and *B* stars that hydrogen and helium are most abundantly revealed. We do not, in view of the success of Saha's theory, believe that this is a complete explanation of the spectral sequence, but it is easier to do so than to reconcile the sequence with a continuous synthesis of atoms. And if

it be assumed that the synthesis is complete, or nearly so, before the stellar material reaches the giant *M* stage, the problem appears to arise again in the question: in what form does the energy exist when the material becomes a star, and what mechanism releases it at a rate just sufficient to maintain approximate equilibrium in the manner pictured by Russell? Unless the building up of atoms takes place during stellar life, there appears to be no advantage in assuming that it takes place at all.

The strong point of Eddington's hypothesis is the fact that the required energy does actually exist in the atoms, and needs super-mundane conditions to release it. Apparently the only other possible receptacle of energy is the ether. We have to account for the source of a star's radiation, but there is also the problem of its destiny. What becomes of the energy which is unceasingly radiated into space? If space is infinite, the problem does not exist: the radiation which is not absorbed by matter travels on to infinity. But if, as we are now coming to believe, space is finite though boundless, and the heavens are actually "rolled together as a scroll," as Isaiah saw them, the ether must be gradually accumulating energy which, in some unknown form, is either transfused more or less uniformly throughout its whole extent or concentrated in solitary places. If the former alternative be true, it may be that, in the interiors of stars, circumstances exist which bring about a transformation of the etherial energy from the unknown form into radiation again. We should then have a cycle of energy changes, from radiation into the unknown form and back again into radiation, with respect to which the material part of the Universe would act as the directing agency.

Speculations on these matters are inevitable so long as human minds are what they are. The problems of the origin and destiny of energy have an interest and a fundamental importance, not only for science in all its branches, but also in connection with the necessities and amenities of life and possibly the very continuance of civilised existence. From the purely astrophysical point of view, however, one of the chief consequences of knowing the source and mode of supply of stellar radiation would be the possibility of estimating with precision the rate of evolution of a star and the duration of its existence in the various stages of its career. We might also be able to interpret more completely the phenomena

of novæ. Even with the rudimentary knowledge we have now, we can perhaps throw a little more light on these mysterious bodies. If high temperature is the necessary condition of release of Russell's unknown source of energy, then we must expect the release to occur in novæ, though here, since the heating is probably on the surface instead of at the centre, the equilibrium state which characterises a normal star could not be set up. Exactly what would happen we cannot, of course, say, but we might reasonably suppose that the expansion of the star giving rise to the second set of absorption and emission spectra (p. 278), and ultimately to the discs seen in the later stages of certain novæ, are consequences of the sudden unloosing of the unknown store of energy when the necessary temperature is reached. Energy released at the surface of a star in this way would be expected to appear as kinetic energy of outward expansion rather than as heat, and this might afford an explanation of the fact that the second spectrum is unaccompanied by an outburst of light comparable with the initial blaze. If such an explanation is adopted, the observed course of events is seen to be quite in keeping with Eddington's hypothesis. Assuming that the release of energy is accompanied by the conversion of hydrogen into helium, we should expect the second nova spectrum to differ from the first in having weaker hydrogen lines and stronger helium lines. This is just what happens: a spectrum of type *A* is succeeded by one of type *B*. How the star passes through its subsequent stages, in which it appears spectroscopically as a planetary nebula and then as a Wolf-Rayet star, it is impossible yet to work out in detail.

To sum up, then, we have no settled ideas about the history of cosmic bodies previous to the earliest giant stellar stage. An attractive case can be made out for the evolution of a nebula of some kind into stars, and we have been able to define some of the conditions which the energy liberated in a star must satisfy, but of definite knowledge we have obtained very little. It is useless at present to speculate on the possible parentage of a nebula. Only the ether is available, and unless matter has existed for all time, we can only suppose that it has been formed out of the ether by a process beyond our conception. We must pass now to the second part of our problem—the past history of the structure of the Universe.

The investigation of the Universe in the past suffers from the serious difficulty that there is so very little certainty about our knowledge of the Universe in the present. We have considered two attempts to formulate a more or less comprehensive cosmology—the theories of Kapteyn and Shapley. We have seen that these two attempts result in hypothetical systems showing features which are unreconciled with one another. It is obviously impossible, therefore, to work back from a single, clearly defined starting point. We must consider the two systems separately, and try to deduce from each the most probable conditions that have preceded and given rise to it. It will scarcely be necessary to say that any ideas which we may form at present on these matters must be regarded as little more than guesses at the truth.

Kapteyn assumes that the stellar system is in a steady state, and therefore that its general shape and the distribution of the motions in it are not changing now, and have not done so for some quite indefinite period in the past. We are at once precluded, therefore, from forming any idea at all of how long ago the Universe, as Kapteyn pictures it, was in any previous state we may assign to it: the steady state may just have been attained, or it may have existed for ages. The problem before us is consequently restricted to a qualitative study of the Universe, and cannot, like the problem of stellar evolution, involve any difficulty arising from the super-abundance of time for the execution of the processes we are led to imagine.

Perhaps the most striking feature of Kapteyn's universe, from our point of view, is the presence of the two systems of stars rotating in opposite directions, which are held to be responsible for the appearance of star-streaming. It is difficult to imagine that a condition of this kind could have developed in a single system: it suggests strongly that our present Universe is a blend of two originally distinct universes which have long been thoroughly intermingled. The shape of the system invites analogy with a globular cluster, and we may suppose, in fact, that we are in the midst of a union of two clusters with the same essential characteristics as the globular ones. But there is a vast difference in dimensions and compactness between our Universe and a typical globular cluster. Our system is very much larger, and its members are very much farther apart. This, however, makes no difference to the main conception.

Jeans has given a number of reasons for supposing that the stars in our Universe were once very much closer together than they are now. If that is so, we may fairly regard them as forming a globular cluster in a more advanced stage of development than the others we know, and built on a larger scale. Tracing the structure still farther back, we come, in Jeans's view, to a spiral nebula, which developed into the cluster in the way we have already described. According to this idea, a spiral nebula is not an island universe, but the germ of one. It seems to be impossible to say when the union took place between the two systems of which our Universe is at present composed. Possibly two newly-formed clusters encountered one another in space, and found it impossible to separate. In the compact condition which would characterise their early days, they would not be able to pass through one another as the moving clusters travel now through the space between the other stars of our system.

The Universe thus appears, on this hypothesis, to contain only two types of large scale organisation—spiral nebulæ and star clusters. The former develop into the latter, which gradually expand and preserve, in their galactic planes, the planes in which the original spirals lay. As we go back in time, we witness a gradual consolidation of our system and of the globular clusters; clusters pass gradually into darkness, and then into spiral nebulæ; spiral nebulæ change into elongated, globular and irregular nebulæ; beyond that we cannot go.

The present relations of globular clusters to the galactic system hint at the possibility that our system will in time absorb the clusters. If so, it may have absorbed others in the past. There seems to be no necessity to suppose that our Universe comprises only two originally independent systems. It may contain a considerably larger number, as its dimensions and the number of stars in it strongly suggest. In the steady state, there can be only two directions of revolution round the axis, however large a number of systems are concerned. It may be, therefore, that several spiral nebulæ have contributed to make our Universe what it is.

There is, in this account of our past, no sign of an original unification of systems—rather the reverse. Each cluster becomes one or more nebulæ which, in their turn, degenerate individually into simpler forms. But we cannot trace these forms back to a common origin. The number of organisations

in space appears now actually to be less than it has been in the past, because of the union of separate systems into larger aggregates. Whether the irregular nebulæ are separate, independent creations of matter, or have been eternally self-existent, or are fragments of a single original mass of matter or proto-matter, we cannot say. Undoubtedly, if the theory that they may develop into star clusters represents the truth, they must be ordinary material bodies now. They emit radiations, and it is by supposing that they are subject, internally at least, to the ordinary laws of motion and gravitation, that the course of their evolution into star clusters is traced.[1] If the observed recession of spiral nebulæ from our Galaxy denotes a dissipation of the whole system of spiral nebulæ and non-galactic nebulæ in general, we cannot, from our present point of view, regard this as an indication that gravitation is not at work among these bodies. There must be some unknown force of repulsion, or else, possibly, the dissipation is the legacy of a huge disruption, in the childhood of matter, of a single parent mass. In the latter event, the apparent repulsion would be merely the effect of inertia, and might or might not in time be overcome by the persistent influence of gravitation. But these are mere conceptual possibilities, for or against the reality of which no evidence is available.

The similarity between our Universe and a globular cluster breaks down almost completely when we adopt Shapley's view of the galactic system. We have then to deal, not with a well-organised system in a steady state, but with a collection of bodies which, as a whole, shows no sign of coherence save the remarkable regularity of its configuration with respect to a central plane. We may, however, retain the general theory of evolution through the earlier non-galactic nebulæ to the spirals and thence to globular clusters (though Shapley, as we have pointed out, regards the spirals as products of a different order of evolution from that of the stars), but we must suppose also that, from some unknown cause, the clusters, when formed, move towards one another and become intermingled in a very flattened aggregate. We have, therefore, essentially the same process as that which we deduced from Kapteyn's idea, but the result to which it leads is different.

From Shapley's point of view we must probably suppose

[1] See, however, footnote on p. 335.

that the formation of the galactic system has taken place gradually, so that the repeated disturbances arising from new arrivals have prevented anything like an organised motion of the system as a whole. Observations, however, have been made which might be interpreted as an indication that the Milky Way itself is rotating. If such a rotation is real, then there does exist an organised motion, though it is not nearly so conspicuous as the movements of the component clusters. There is certainly not even the roughest approximation to a steady state in the system which Shapley describes, except as regards the internal motions of separate parts.

The present army of globular clusters would appear to be the remnants of a larger company, of which the forward members have already been absorbed in the equatorial segment. But we cannot completely explain the equatorial segment in terms of erstwhile globular clusters alone. Its extreme flatness—the axial ratio is 20 or 30 to 1—seems to require an additional explanation. Possibly there was originally a nucleus of some kind, already extremely flat when the globular clusters began to be drawn within its boundaries. If so, future work on the analysis of the "field" may bring its existence and character to light. At present we know nothing of it.

In the dissection of the Universe into systems which were originally distinct, the study of stellar motions will probably play the greatest part. However much the direction of movement of a cluster has been modified by absorption in the galactic system, the stars in it appear still to retain in large measure the common motion due to the original internal organisation of the cluster. The intrinsic qualities of the stars may also be of assistance. Stars born at different times, in different places, and possibly under different conditions, may show distinguishing characteristics which will lead to their classification. Such characteristics would probably be found, if anywhere, in the spectra. The stars of any of the rarer spectral types, for instance, may owe their peculiarities to special conditions existing in the common group to which they might originally have belonged. Or there may be subtler variations in spectra now classified together, which may bring to light differences of origin of the stars. Shapley's conception of the Universe opens up a wide field of research in the study of cosmic history, which will doubtless be explored as far as possible.

The condition of the Universe in the past is a subject on which it has only just become possible to speculate. All our ideas on the matter are necessarily tentative, and must remain so at least until we have a more definite and firmly-established conception of the structure of the Universe in the present. In the vague and uncertain guesses which we are now able to make, we find no hint of a beginning of things or of a cycle of changes. Step by step we go back along a dubious way until we are arrested at what seems to be the earliest form of matter of which specimens still exist. Is an irregular nebula the original material body? If so, we have no conception of how it was formed. Unless we exist at a special point of time in a Universe which had a beginning in time—an assumption which we are as loth to make as the corresponding one, that we are at a particular place in a finite space—the whole process of evolution must be going on now as always, and the creation of matter, if matter ever was created, must even now be taking place. Our knowledge of a distant body begins when the body can radiate or gravitate. The discoveries of Uranus and Neptune are the types of all possible discoveries. If the properties of radiation and gravitation are not eternal in the stuff of which matter consists, our journey into the past must find a limit so far as the physical senses are concerned. Whether or not our subtler powers can pierce beyond the limit is at present a metaphysical question, into which it is not the business of astrophysics to enter.

CHAPTER XX

AS IT WILL BE

"We know what we are, but know not what we may be." Whether or not we are willing to grant the first part of Ophelia's dictum, the second part is beyond question—with regard either to ourselves as individuals or to the Universe we inhabit. We might go further, and say that it is impossible to forecast even the most probable destiny of the Universe. The old mechanistic view, which saw in Nature nothing but matter and motion and wanted but a knowledge of the initial positions and motions of bodies, and perfect mathematical skill, in order to predict the course of events for all time—that view has faded into obscurity. Without necessarily denying the possibility of expressing in simple terms the state of the Universe at any future time when its state in the present and the whole of its contents and their properties are known, we realise now that we do not by any means know the whole of its contents and their properties. We have but lately discovered the importance of radiation pressure, for instance; we have to admit an "unknown source of energy" in a star; we meet with variable stars to whose variation we can assign no cause; we do not know why spiral nebulæ retreat from the Galaxy; and he would be bold indeed who would venture in these days to assert that Nature has not still other agents, labouring in unknown ways at processes yet undiscovered. What becomes of the energy which the stars radiate into space? This question alone, so long as it remains unanswered, is an impassable barrier to our certain knowledge of the future.

And yet, if there is any truth at all in the principle of continuity; if the theory of evolution which we have sketched is anything but a complete illusion—we must be able to see some little distance beyond the present moment, though it be as through a glass darkly. We can surely say that, unless some accidental catastrophe should occur, Betelgeuse will be, in the next stellar age, what Aldebaran is in this. If not, we know nothing beyond the bare facts of observation. It is

impossible to doubt that the configuration of the visible Universe will change—for a time, at least, in the direction indicated by the present movements of its parts. That also is as certain as anything in this mysterious world. To a large extent the problem of the future is the problem of the past, and they are both summed up in the problem of the present. The doctrine of evolution implies that, at any instant, the Universe contains specimens of bodies in many, if not in all, stages of development. The past of one body is the future of another. We can, at any rate, follow the development of each body up to the last stage in our conception of the course of cosmic evolution.

The difficulty begins when we try to extend our conception beyond the limits set by the data of observation. An unbroken sequence of changes is established from the giant *M* stars, through the series of Harvard types and back again to the dwarf *M* stars ; but before the *M* giants and after the *M* dwarfs we have to find our way by intuition or pure reason ; there is no succession of observations to guide us. We have traced a possible line of development from the non-galactic nebulæ to the stars, in which all known types of body find a place except the diffuse nebulæ and the subordinate members of our solar system. Such a generalisation is most satisfactory from one point of view, but it extends our knowledge only in one direction—towards the origin of things : nothing is left us to mark the path to their destiny but the diffuse nebulæ, for the members of the Sun's family are precluded—on grounds of mass, to mention only one thing—from a place in the line of posterity of the stars. And there seems to be little possibility of regarding a diffuse nebula as representative of the post-stellar condition. Its characteristics are, in some respects, quite the opposite of those of the late type dwarf stars. If a dead star becomes a diffuse nebula, the process by which the metamorphosis takes place is anything but obvious. Nor are we in a better position if we assume that stars are formed from diffuse nebulæ instead of from spirals. We have then the non-galactic nebulæ left over, and these are as difficult to place after the stars as are the diffuse varieties. We are forced to the conclusion that whatever immediately succeeds a dwarf *M*-type star in the course of cosmic evolution is a body of which we have obtained no evidence by observation.

Let us try to imagine what a dying star might become

on the supposition that there is no break in the continuity of development after the latest stage of which we have any knowledge. The decreasing luminosity with advancing type among dwarf stars may well explain why we have no record of post-stellar matter: presumably it exists, but is too faint to be seen. There must, therefore, be an abrupt cessation in the observed sequence of spectral type and colour, for these qualities characterise luminosity. The velocities of the ex-stars should be fairly high, for the bodies will have relatively small masses, having in general passed through their stellar existence more quickly than the still luminous stars. This does not preclude the possibility that there are some very massive ones among them which were born much earlier than the rest. Their galactic concentration should be relatively small. Their effective temperatures should be well below 3000°, and their densities should be high.

Superficially, at least, these considerations point to the continuous degeneration of a star into a cold, ultimately solid body, whose feeble radiation into space is just equal to the radiation it absorbs from the rest of the Universe. Such a state would appear to be final: we know of no subsequent development that can take place. If, then, the process of creation of stars has been going on for an infinite time—or even for a time very long compared with the lifetime of a star—space should be thickly populated with dead worlds. Actually, however, as Macmillan has pointed out, the largest certainly solid body of which we have any knowledge is the Earth. Allowing for the fact that dark bodies would necessarily be hard to perceive, we should still expect to find some evidence of them, either by observation or by their effect on the movements of bright stars. Yet all the evidence available goes to show that the number of dark bodies in space is not excessive. We know of practically no instances in which bright stars have become obscured in a manner explicable by the accidental passage of dark bodies across the line of sight. According to Kapteyn's dynamical theory of the Universe, the total mass of dark matter is small. And, if Kapteyn's view be regarded as inadmissible in the light of Shapley's researches, the density of bright matter in the Scorpio-Sagittarius region (Plate XLVI)—the direction in which the Universe has its greatest depth—is equally against the existence of a surplus of dark stars. Facts, as opposed to speculations, suggest either that—in the portion of the spatial

universe within reach of our telescopes, at least—the process of creation of stars has been going on for a time not greatly exceeding the duration of a star's luminous career, or else that the dense, dark body state occupies at most but a brief interval in the evolution of stellar material. Neither alternative presents a very prepossessing appearance.

If we look at the matter more closely, however, we may find reason to doubt whether, after all, the present condition of a dwarf *M* star points so directly towards a dense, dark future. The average density of an *Ma* dwarf star is 5.4—as great as the mean density of the Earth. Now we know that the mean density of the Earth's crust is only one-half of this, so that the internal parts must have a considerably higher density—as we should expect, from the great pressure to which they are subjected by the weight of the crust. A star also must be very much more dense at the centre than at the surface, and, owing to its relatively enormous mass, the variations of density must be greater than those of the Earth. The interior of a late type dwarf star must therefore be extremely dense. On the other hand, its temperature must be extremely high: in the atmosphere it is sufficient to vaporise refractory metals and compounds, and at the centre it must be measurable in millions of degrees. The matter for a considerable distance around the centre of a late type dwarf star is probably much denser and almost incomparably hotter than anything we have experienced on the Earth. We know nothing of matter in this state. The effect of high temperature is to vaporise a substance: the effect of great pressure is to liquefy or solidify it. In the stars we are dealing with, we have the two agencies working with terrific strength one against the other. To liken the circumstances to a meeting of Greek with Greek is to show how impotent terrestrial struggles are to represent the antagonism of these natural forces. It is the nearest possible approach to the old paradox of an irresistible force encountering an impregnable obstacle.

Of course, the same kind of thing goes on—perhaps in a lesser, but still enormous degree—in the dwarf stars of earlier type. The conditions there far transcend human experience, yet nothing untoward happens—there is no break in the continuity of development. But the course of evolution through the Harvard types is extremely slow. The star has time continuously to adjust itself to the changes of temperature

and density, and it can remain always in a state of equilibrium —mainly radiative. The case is quite different when we come to the end of the Harvard sequence. We are then approaching the circumstances in which the atmosphere and the surface of the star are about to condense, and condensation is essentially a discontinuous process. On one side of a critical point, a substance is a gas : move ever so slightly to the other side, and it is a liquid or a solid, with a density suddenly increased by many hundreds per cent., and—what is perhaps still more important—a sudden change from transparency to opacity. This cannot but upset the equilibrium of a star. Radiative equilibrium is no longer possible at all, because the outer layers are opaque to most kinds of radiation. A tendency towards *conductive* equilibrium must set in, and how the star in its unparalleled condition will deport itself under this necessity can perhaps scarcely be conjectured. We can at least say that an uninterrupted degeneration to a totally solid state is one of the last things to be expected.

Let us suppose, as a working hypothesis, that an explosion of some kind takes place, in which the imprisoned energy—material and etherial—bursts through the newly-formed crust. There should be many examples of such a process in the sky, and we might expect to find some of them. We do, as a matter of fact, find bodies which might fulfil the conditions so far as appearances are concerned—there are long-period and irregular variables, and novæ—but it does not seem to be possible to identify any of them with the hypothetical bodies under consideration. The variables appear to be mostly giants : some novæ, also, whose general changes are absolutely typical, were not of the latest spectral type before outburst, and the almost complete galactic concentration likewise rules them out of account for our purpose. We must conclude, then, that if post-*M* dwarf stars explode, we do not witness the explosions. We need not, however, abandon the hypothesis on that account ; we may suppose that the luminosity of the explosion is too faint to be visible at stellar distances. This is a plausible assumption, for the bodies in their last stellar stage are very faint, and the cooling produced by an explosion, as in the similar circumstances attending a sunspot, would probably darken them still more. Our failure to see them may be due, not to their non-existence but to the limitations of our senses.

The result of the explosion might be calculable if we knew

the precise conditions inside the star. As we do not know them, we are at liberty to make any assumption that is not inconsistent with the little we do know. If we assume that the catastrophe is thorough-going and bursts the star into dust and vapour, we can construct a bridge from the dwarf stars to the diffuse nebulæ, and classify the latter as the offspring rather than the parents of stars. We cannot say that this is impossible so long as the heat energy inside the star is sufficient to account for the gravitational potential energy of the nebulæ, and there is every reason to believe that it is sufficient. We have, in fact, a modified reversal of the process by which the heat of a star is developed through contraction.

Two alternatives are now open to us in constructing a general course of cosmic evolution. We can start with the irregular nebulæ, pass through the other non-galactic forms to the spirals, thence to the giant *M* stars and Russell's succession of stellar ages, arriving finally at the diffuse nebulæ: or we can begin with diffuse nebulæ, pass through the same stellar sequence, and return to diffuse nebulæ again at the end, regarding the non-galactic nebulæ as representatives of a second independent order of material development. Each alternative has its own distinctive merits. The first finds a place for every kind of body known (except, of course, planetary nebulæ), and throughout the whole of the course of development there is no stage unrepresented by observed bodies except the very brief periods immediately before and after the succession of luminous stellar stages. There remain, however, the unknown beginning and end: there is no sign of what precedes the earliest non-galactic nebula, or of what follows a diffuse nebula. The second alternative apparently overcomes this difficulty, but it leaves undetermined the character of the second order of evolution followed by the non-galactic nebulæ. It points, furthermore, to a fundamental rift in Nature, for it would appear impossible, if the diffuse nebulæ and stars take part in a cycle of changes, ever to find a common origin for the bodies now following different lines of development.

But we must hasten to say that a cycle of changes by no means follows from the assumption that a diffuse nebula develops into a star and a star into a diffuse nebula. To establish such a cycle we should have to show that the nebula preceding a star was identical in every respect with

the nebula following a star. All that our hypothetical course of events implies is that the two bodies may be alike in density, temperature, luminosity, and possibly chemical composition. Whether they are alike in possessing the all-important power to develop into stars is quite another question, which must be answered, so far as we can see, in the negative. There will be two classes of nebulæ—" potent " and " impotent " we may call them, for the names *giant* and *dwarf* are not applicable here. The fundamental distinction between them arises from the actions of a star during its lifetime.

A star may be defined as an instrument for broadcasting energy into space. The energy it emits is either entirely developed in it by contraction, or else is largely drawn from an unknown source whose properties we have already discussed.[1] In the former event, the passage from a star back to a nebula would be impossible, for there would not be sufficient energy in the star to bring about the transformation. In the latter event, which is by far the more probable, the transformation might occur, but the nebula would have lost its power to command the unknown source—otherwise the star would not have died out. However we look at the matter, therefore, a never-ending cycle of changes seems impossible at present to establish.

The difficulty, from our present point of view, appears quite clear. On the one hand, energy comes from an unknown source ; on the other, it is sent out through space to an unknown destination. If the source and destination could be identified, the cycle would be established. If we suppose, in order to fix our ideas, that the source of energy in a star is, as Eddington suggests, the building up of the heavier elements from hydrogen, then the purpose of Nature in the material world seems to be the conversion of hydrogen into radiation and other elements. We are still inclined to regard the other elements as permanent bodies, so that we have a course of evolution whose continuous effect is the transmutation of hydrogen into radiation. To complete the cycle it would be necessary to suppose that radiant energy is concomitantly changing into hydrogen. Macmillan has put forward a hypothesis of this character—without,

[1] Certain minor accessions of energy, such as the absorption of radiation from other stars and of kinetic energy of bombarding meteors, are here ignored.

however, suggesting how or where the change takes place.[1]

It is difficult to conceive of a process that quite transcends ordinary experience. In these days, however, when we are ready to change space into time and electricity into matter, this particular form of transubstantiation need not scare us on that account. But we must notice that, even if it is real, our difficulties are by no means over. Let us start our imaginary cycle with a nebula of hydrogen. It forms stars, which ultimately become nebulæ again—composed, however, now partly of heavier elements. The energy released during their synthesis appears as radiation, and is ultimately converted again into hydrogen. But we have now less hydrogen than we had at the beginning, because a considerable part of the original supply has been changed to other substances. The process is repeated, with a further change of some of the hydrogen into other substances. This goes on—probably only a few times—until very little hydrogen is left. The star must then depend chiefly on the heat of contraction for its maintenance, and it therefore runs through its life quickly. It radiates part of this heat, and so has not sufficient left to enable it to change back into a nebula again. There seems to be no alternative to the dense, dark body state, reached however, not after one stellar life, but after a series. The whole aim of Nature would thus be to form the chemical elements out of hydrogen.

Our imagined cycle therefore still eludes us. Matter has increased its average atomic weight and density, and changed its chemical properties ; gravitational potential energy has disappeared, and in its place there is either a small equilibrium distribution of radiant energy in space, or a surplus of matter due to the conversion of this radiant energy completely into hydrogen. With respect neither to matter nor to energy is there any obvious possibility of further development, except perhaps a coalescence of the dead stars into larger and larger bodies. The line of thought we have followed seems to lead inevitably to the conclusion that the whole creation is moving towards a far-off event, though it needs a considerable amount of pessimism to call it divine. It should scarcely be necessary, however, to point out again how exceedingly rash and uncertain is any attempt at all to look into the distant future with our present eyes. We speculate because

[1] A similar idea has lately been advanced by Lodge.

it is not possible for human nature to avoid doing so, and indeed, speculation—so long as we do not delude ourselves into thinking it irrefutable logic—is a very salutary occupation. But we have had too much experience already of the extent of Nature's resources not to know that they reach far beyond our present comprehension.

The final structure of the Universe is as difficult to forecast as the destiny of individual bodies. So far as the immediate future is concerned, we are probably justified in supposing that present tendencies will continue: the spirals will recede farther and farther from the Galaxy, and the globular clusters will, on the whole, approach it. Shapley points out that a cluster will probably pass more than once through the equatorial segment, oscillating to and fro until it is finally absorbed: the disruptive forces during a single passage would be incompetent to convert the system into an open cluster. If the whole company of clusters becomes absorbed, and no others move up, the Galaxy will probably proceed with extreme slowness to approach a steady state, becoming finally a system with the same general characteristics as those which Kapteyn ascribes to the Galaxy now. How such a system would ultimately behave, we cannot say. It might remain eternally performing its steady motions when the stars are dead, cold globes, or there might be an extremely slow consolidation, leading to a fusion of the whole Galaxy into one or a few huge bodies. But we can form no conception of the properties of extremely large dense masses: we do not know that it is possible for them to exist.

We have perforce left out of account all special types of body—novæ, Wolf-Rayet stars, planetary nebulæ, and the stars of the rarer spectral types. It seems clear that there is no place for these in the main line of development. The red stars of the rarer types, which probably lie along branches of the Harvard sequence, apparently present no special difficulty with regard to the future: they are giant stars, and there is every reason to believe that they will merge into the normal series of types before their development proceeds much further. Novæ, if our theory of their origin is true, can come into existence only so long as there are diffuse nebulæ in the Galaxy. If there ever comes a time when every such nebula has become transformed into a star, and every star is cold and dense, the age of novæ will be past. There will, of

course, be stars which have been novæ. We must probably assume that they also will become cold and dense, though we have neither observational nor direct theoretical justification for the assumption. We have followed novæ to the state of faint variable Wolf-Rayet stars, and no further. If, in some way, they grow into planetary nebulæ—a form which they cannot retain for long, or we should find more planetary nebulæ than we do—we may be wrong in assuming that they degenerate into the same type of body as do ordinary stars. At any rate, even if they do so degenerate, the path may be much more tortuous than we can imagine. So long, however, as the history of a nova remains so little understood as it is, nothing is to be gained by bringing these bodies into our plan of the universe of the future.

Our story is told. From the standpoint to which we have attained, we have looked around on the conquered country and forward into the regions yet to be explored. It is not the fashion nowadays to point the moral, nor, if we were able, would we wish to do so, knowing that, of the vital meaning of things,

> "No secret can be told
> To any who divined it not before."

But perhaps we may be permitted, in conclusion, to point to what is not the moral. When the heavens were first opening out to the mind of man, and distances of which we could form no conception were dwarfed into points by immeasurably greater distances; when the globe which is our home and our prison was seen to be all but the tiniest of millions and millions of globes; and when, in the whole Universe, no voice spoke to the inquiring mind save that of vastness and fixity and majesty—it was perhaps inevitable that, from the sense of our littleness, we should gain a feeling of transience and unimportance. That conclusion, sublime as an emotion but abhorrent as a thought, has crept into our philosophies and even into our language, so that "greatness" is a metaphor of "nobility," and "smallness" is synonymous with "insignificance." It is time that we adjusted our eyes to the new outlook. In the realm of astrophysics, the slow and the quick, the small and the great, are inseparable. Where there is a stellar age, there is also a light wave; where there is a

globular cluster, there is also an electron, and every increase in distance of the one reveals yet more power in the radiations of the other. The real value and meaning of things are not to be found in space and time measurements. Apart from the space-time framework in which, according to our circumstances, we clothe the objects of our perceptions, are the events of Nature themselves, which are not limited by the properties of the medium in which they are seen. It is not easy to shed the prejudices of generations, but if we can rise to the conviction that boundlessness is not infinite extension, and immortality is not eternal existence, we shall be in sight of the ground towards which the astrophysics of the future is likely to march.

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Dr. J. Evershed. Plates xxi., xxviii.

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Harvard College Observatory. Plate ii.

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